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THE SOURCES OF ANATOMICAL LITERATURE

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THE study of anatomical literature has not received the attention that has been given the writings of men in other lines of intellectual endeavor. When we compare, for instance, our knowledge of the literature of anatomy, and the men who have made this literature, with the work that has been done on the history of poetry and the poets, or fiction, or the history of nations, we see how greatly the development of anatomical knowledge and literature has been neglected. Locy[1] especially has shown us how this field of study may be used as a field of research in early human documents relating to anatomy. The subject has been further developed by Stieda, Holl,[2] Sudhoff,[3] Forster,[4] McMurrich,[5] Roth,[6] Töply[7] and others who have contributed sundry studies along these lines. That there has been no great amount of attention paid to the subject is probably due to the fact that the subject matter of anat-

[1] *Journal of Morphology*, Vol. 22, pp. 945–988, 1911.

[2] *Archiv für Anatomie und Physiologie*, Anat. Abth., Jahrgang, 1905, p. 96.

[3] Karl Sudhoff is editor of the *Archiv für die Geschichte der Medizin*, to which he is an active contributor (especially to be noted are his original contributions concerning medieval anatomical knowledge) and he is also the editor of Pagel's "Einführung in die Geschichte der Medizin."

[4] *Archiv für Anatomie und Physiologie*, Anat. Abth., 1904, pp. 372–384.

[5] *Medical Library and Historical Journal*, Vol. 4, pp. 338–350, 1906.

[6] *Archiv für Anatomie und Physiologie*, Anat. Abth., 1905, p. 79; 1906, p. 77.

[7] Anatomische Hefte, Bd. 25, Erste Abth., pp. 351–398, 1904.

omy has been and is of more interest than the form in which it is presented.

There have been a number of classical studies in the history of anatomy, the latest and best of which is that by Töply.[8] Other and earlier studies to be mentioned are the productions of Lauth,[9] Haller,[10] Portal,[11] LeClerc and Manget,[12] Tarin,[12a] James Douglas,[12b] Goelickius and many other early attempts at bringing together the results of anatomical study.

More recently the work of Daremberg in France,[13] Carus,[14] Wieger,[15] Weindler,[16] and Hopf[17] in Germany; Osawa[18] in Japan, Bardeen[19] in America, and Chievitz[20] in Denmark are to be especially mentioned.

Besides the results contained in the above-mentioned works there is much information to be gleaned from the numerous histories of medicine and especially from the

[8] Geschichte der Anatomie, in "Handbuch der Geschichte der Medizin," begründet von Th. Puschmann, herausgegeben von Max Neuberger und Julius Pagel, Bd. II, pp. 155–326, 1903.

[9] Thomas Lauth, 1815, "Histoire de l'anatomie," Strassburg. Up to the time of Thomas Bartholin, 1671.

[10] Albrecht von Haller, 1774–1777, "Bibliotheca Anatomica," Tomes I–II.

[11] Antoine Baron Portal, 1770–1773, "Histoire de la anatomie et de la chirurgie," Paris, Tomes I–VI.

[12] Daniel LeClerc and Jacob Manget, 1685, "Bibliotheca Anatomica."

[12a] Pierre Tarin, 1753, "Dictionaire Anatomique."

[12b] James Douglas, 1715, "Bibliographiæ Anatomicæ specimen, seu catalogus omnium pene auctorum, qui rem anatomicum professo vel obiter scriptis illustrarunt."

[13] Ch. Daremberg, 1870, "Histoire des sciences médicales comprenant l'anatomie, la physiologie, la médicine, la chirurgie et les doctrines de pathologie générale." 2 tomes.

[14] J. Victor Carus, 1872, "Geschichte der Zoologie, bis auf Joh. Mueller und Charl. Darwin."

[15] Friedrich Wieger, 1885, "Geschichte der Medizin und ihrer Lehranstalten in Strassburg vom Jahre 1497 bis zum Jahre 1872."

[16] Fritz Weindler, 1908, "Geschichte der gynaekologisch-anatomischen Abbildung."

[17] Ludwig Hopf, 1904, "Die Anfänge der Anatomie bei den alten Kulturvölkern." Abhandl. zur Geschichte der Medizin, Heft IX, Breslau.

[18] Gakutaro Osawa, 1895, "Zur Geschichte der Anatomie in Japan." Vortrag gehalten in der Naturforscher-Ges. zu Freiburg i.B. am 20, Nov. 1895. A.A.B. 11, N. 16/17, pp. 489–504, 2 Abb.

[19] Charles R. Bardeen, 1905, "Anatomy in America," *Bulletin of the University of Wisconsin*, No. 115.

[20] I. H. Chievitz, 1904, "Anatomiens historie," Copenhagen.

biographical dictionaries of Panckoucke,[21] Gurlt und Hirsch,[22] Pagel,[23] and the various biographical encyclopedias. Much valuable biographical data of many biologists is to be found in the "Nouveau Larousse Illustré," as well as in other general encyclopedias.

Even a hasty survey of the geographical development of anatomical literature will suffice to show that the continents of Europe and North America are the chief ones to be considered. Asia, Africa, Australia and South America each come in for some slight claim to attention, as will be evident from the discussion of the geographical distribution of anatomists given below.

It can not be said that all of the men considered in making up the list referred to below have contributed new ideas to anatomy. Many have not. None of the Romans were men of original ideas, at least so far as a knowledge of anatomy is concerned. Celsus is the only Roman whose knowledge of anatomical subjects demands any sort of respect and his knowledge, as given in the "De Medicina," is not acquired first hand although he is to be greatly respected for producing a medical classic.

Stieda[23a] has shown, however, that the Romans were not entirely devoid of anatomical knowledge, though this knowledge was often erroneous. The "Donaria" described and figured by Stieda are supposedly offerings to the deity in connection with the suppliant's plea for health. The part offered, in the form of a model of a leg, foot, breast, viscera or head, indicates the region in which the suppliant suffered and from which distress he wished to be relieved. The objects are of marble and bear the date of about the first century B.C. They are, for the most

[21] C. L. F. Panckoucke, 1820, "Dictionaire des Sciences Médicales-Biographie Médicale," 7 volumes.

[22] "Biographisches Lexikon hervorragender Aerzte aller Zeiten und Völker," 1884–88, 6 volumes.

[23] "Biographisches Lexikon hervorragender Aerzte des XIX. Jahrhunderts," 1901.

[23a] Ludwig Stieda, 1901, "Anatomisch-Archäologische Studien," II. Anatomisches über alt-italische Weihgeschenke (Donaria), Anatomische Hefte, Bd. XVI, pp. 1–84, Taf. I–IV.

part, crudely done and can not be taken as indicating any attempt to illustrate anatomy. Often the internal parts shown bear some resemblance to the human structures. Occasionally the liver of a mammal is incorporated in the same piece with the human heart and lungs. The viscera are very crude and can not be taken as indicating any degree of positive knowledge concerning the parts shown.

None of the Arabians produced original ideas or literature concerning anatomy. Abdollatif (1162–1231) is the only one of the Arabians who departed in the slightest degree from the writings of Galen and Hippocrates. While in Egypt he was studying some human bones in a cemetery when he ascertained that the jaw is formed of one piece; that the sacrum, though sometimes composed of several, is most generally of one. On the basis of these observations he criticized the writings of Galen and thus showed himself to be a man of original ideas.

Flores[24] in his "History of Medicine in Mexico" has listed the following teachers of anatomy in the University of Mexico: Febles, Benítez, García, Cheyne, Peña, García Cabezon, Rendon, Escobedo, Villar, Jecker, M. Andrade, Muñoz, Villagran, Durán, F. Ortega, Chacon, Montes de Oca, Velasco, San Juan, and Cordero é Icaza, but, so far as I have been able to ascertain, none of these men have been productive.

There is no Egyptian literature of anatomy, and apparently there was no definite knowledge of anatomical structure. The practise of embalming had attained at one period great perfection in Egypt and this may have resulted in a certain degree of anatomical knowledge, but it was so overclouded by religious fanaticism and superstition that it amounted to little. Thus McKay[24a] says: "The Egyptians probably knew next to nothing about anatomy, as their religion forbade dissection, and the embalmers probably learnt little. After the Paraschistes

[24] Francisco A. Flores, 1888, "Historia de la Medicina en Mexico desde la epoca de los Indios hasta la Presente," Tomo I–III.

[24a] W. J. S. McKay, 1901, "The History of Ancient Gynaecology," New York, p. 6.

had made the preliminary abdominal incision, the Taricheutae were accustomed to pass their hands through the incision into the body and remove the heart and kidneys and digestive organs. If they were accustomed to remove the uterus and ovaries, they must have gained some knowledge of the organs, but we have no authority for saying that the uterus was really removed. The custom that the Egyptians followed, that of making models of the parts[24b] that had been healed and then hanging them in the temples, may have been useful for clinical instruction."

Among the Hebrew peoples of ancient times sacrifices (Genesis xv. 9–10) were common, and the appearance of the viscera of the animals sacrificed were probably familiar. This may have resulted in a degree of anatomical knowledge. There are, apparently, no definite statements concerning anatomical matters in the Bible, the information there given being of a purely popular character. In the Babylonian Talmud, however, there are a number of references[25] to subjects of anatomical interest. The number of bones in the skeleton is estimated at 248 or 252, and one of these, the bone Luz, which was supposed to be situated somewhere between the base of the skull and the coccyx, was regarded as the indestructible nucleus from which the body is to be raised from the dead at the resurrection.[26] The Talmud also displays some knowledge of the esophagus, larynx, trachea, and the membranes of the brain. The pancreas and other internal organs are briefly referred to.

Among the ancient peoples who thrived in and around Mesopotamia there is, apparently, no anatomical literature. What little of anatomy may have been known was acquired through religious observances, such as auguries and sacrifices. Stieda[27] especially has studied the indi-

[24b] These were "Donaria" and the custom was doubtless derived from the Romans.

[25] Julius Preuss, "Biblisch-Talmudische Medizin," Berlin.

[26] F. H. Garrison, *N. Y. Med. Jour.*, 1910, Vol. 92, pp. 149–151.

[27] Ludwig Stieda, *Anatomische Hefte*, Bd. 15, pp. 673–720, Taf. 57.

cations of anatomical knowledge among these ancient peoples as this knowledge has been preserved in their sculptures. It is a matter of great interest that he has interpreted a terra-cotta object from Babylon, to which an age of from 2000–3000 B.C. may be assigned, as a model of a sheep's liver, supposedly used in connection with soothsaying or with sacrifices. This interpretation is sustained by the description of two other similar objects of a later date, one in alabaster from Piacenza, and a bronze liver from Settina. One can recognize on the visceral surface of these objects the processus papillaris, the processus caudatus, and the vesica fellea; all of which are very clearly represented. The lower surface of the object from Babylon is divided into squares and studded with inscriptions, supposedly of a prophetic nature.

In addition to these very definite anatomical models many plastic representations exhibit some knowledge of the superficial musculature of the extremities. The larger subcutaneous veins, such as the cephalic, basilic, and saphenous, are often clearly shown. From an anthropological standpoint it is noteworthy that various racial types are indicated in some of the representations of the head, so that we can not say that these peoples were entirely devoid of anatomical knowledge and we are forced to admit their keen powers of observation.

Such anatomy as was taught to students in the medical schools of China was highly erroneous and fanciful. Although the study of medicine has a very ancient history in China, as ancient as the history of its civilization, going back to more than 3000 years B.C., anatomy was not studied at all in any laboratory form. They taught, for instance, that there are 365 bones in the human body; that the small intestines were attached to the heart; that they were traversed by the products of digestion; that the larynx opens into the heart; the spinal cord into the testicles, that the lung has eight lobes; the liver seven, that the kidneys, suspended the vertebral column, have the form of an egg and possess the subtle principle of generating the spermatic

fluid, primarily elaborated by the brain, condensed in the testes and from there conveyed to the spermatic duct. Osteology was somewhat better known, although the skull, pelvis, forearm and leg were regarded as being formed of one bone each, or at times eight bones were assigned to the head in the male and six in the female. Something was known of the tendons and ligaments. The spleen and heart were regarded as the organs of reason. Ancient Chinese medical literature consists of a large number of works, none of which are of any scientific importance. There is no modern Chinese anatomical literature.

What little of anatomy was known in ancient India is contained in the writings of Atreya, a physician who wrote a good description of the bones of the human body, and who is said to have taught in the Taxila University during the sixth century B.C.; as well as in the writings of the surgeon Susruta, of a somewhat later date. As the writings of these men have been interpreted by Charaka, ancient East Indian anatomy regarded the body of man as possessing seven skins, seven elements, 300 bones, 24 nerves, 3 fluids, 107 joints, or 68 movable joints and 142 immovable ones, 900 ligaments, 90 tendons, 40 principal blood vessels with 700 branches. and 500 muscles. The blood vessels and nerves[28] radiated out from the umbilicus as a center. Nothing was known of the courses of these structures within the body.

There is a later publication of about A.D. 800 entitled "Amarakosha," which discusses somewhat the nature of the human body but there is no later treatise which might be termed anatomical, and there is no modern East Indian anatomical literature.

While engaged in a biographical study of the men who have contributed to the advancement of our knowledge of vertebrate anatomy, the writer has been attracted by a number of interesting facts relating to the sources of anatomical literature, which, he believes, are not generally

[28] Haeser, "Lehrbuch der Geschichte der Medizin," Bd. I, p. 18–20.

recognized. The following preliminary study is an attempt to arrive at some conclusions as to the origin and development of our modern anatomical literature. It is presented here in anticipation of further studies along these lines.

The names of the men included in the above-mentioned study are of those anatomists, or contributors to anatomical literature, who are no longer living, and who have contributed in any way to the anatomy of the vertebrates, whether by practical or theoretical studies. The men of all countries and all times have been listed, so far as it has been possible to ascertain them. Doubtless many have been overlooked because their records are in relatively obscure places.

The subjects represented in the present study are: human anatomy, artistic anatomy, anthropology, comparative anatomy, embryology, histology, zoology, vertebrate paleontology and subjects of general interest which bear theoretically on the morphology and evolution of the vertebrates, such as Mendel's work at Brünn, and the work of Darwin, Weismann, Charles Bonnet and Lamarck. It may be contended that these subjects constitute biology rather than anatomy, but biology is certainly the more inclusive term. We may, to be sure, speak of the anatomy of the bacteria and in a sense the bacteriologist is an anatomist, but for my present purpose the names of those men who have contributed to our knowledge of the morphology of the vertebrates will suffice. One of the guides, which has been useful in selecting the names suited for the list, is that of the anatomical terminology. If there are anatomical structures (such, for instance, as Hesselbach's triangle) named for the man, he is included, though this is by no means the only guide. Those men whose writings are of a strictly taxonomic nature are not included, unless important theoretical results have arisen from such writings, such as has been the case with the taxonomic work of Lamarck, Linnæus, Cuvier and others.

It is hoped later to make a comprehensive survey of the development of anatomy. Over one thousand names have been compiled, and will ultimately be studied. Eight hundred of these have already been partially examined and will shortly be published.

In the development of our anatomical literature there has been involved a whole host of men in a number of different lines of activity, which it will be interesting to discuss. Many of the men included in the list were not professional anatomists but since they contributed to the advancement of anatomical knowledge we may regard them as contributors and they are hence deserving of consideration.

As would be expected, the following survey of the anatomists indicates, in general, an intellectual development in each country at the time when other conditions, physical, social, religious and political, favored the growth of mental work among the nation, although this statement finds certain contradictions, as in the case of Michael Servetus, Abano, Fallopio, Malpighi, Vesalius, and many more of the early students especially, whose work was accomplished under adverse conditions. The dates of the majority of the anatomists who find a place in the present study belong to a period when intellectual endeavor had attained a firm place in each country. Among the Greeks for instance, there is no one who attained eminence in anatomy later than the third century A.D. No modern Greek anatomist is included in the list.

There are representative anatomists of twenty-seven nationalities, though many of the countries are not widely separated geographically. The following geographical distribution indicates nothing previously unknown, but is presented here simply as an interesting survey. There are doubtless many more men of all these nations who are deserving of mention. The list contains 1 Japanese (Mitsukuri, 1858–1909, an embryologist, who is here regarded as the only one of this nation who has occupied a high place in anatomical work); possibly also the zoolo-

gist Nishikawa and the anatomist Taguchi should be mentioned; 1 Armenian (Alcana Mosali, who in the thirteenth century wrote a treatise on the anatomy and diseases of the eye, chiefly compiled from Arabian, Chaldean, Jewish, Greek and other sources); 1 Hungarian, 1 Polish, 1 South American (Florentino Ameghino, 1854–1911, a student of vertebrate paleontology, is the only representative of the large South American continent in the list. Ameghino's attainments in vertebrate paleontology entitle him to a high place among the anatomists of the world); 1 Turk (Schanzi Zadeh Mehemmed Ataullah, a Turkish physician, who after completing his studies in Italy, published, in 1820, a work on human anatomy, in folio, illustrated with 56 copper plates); 2 East Indians (Atreya, a physician, who was a teacher in the Taxila University in the sixth century B.C. He wrote an osteology, which was later edited by one of his students, Charaka. Susruta, an East Indian surgeon also deserves mention), 2 Bulgarians, 3 Flemish (of whom the greatest was Vesalius), 4 Romans (none of them men of originality), 5 Russians, 5 Belgians, 7 Irish, 7 Swedish, 7 Spanish, 7 Bohemians, 9 Arabians (Abdallatif, Albucasis, Avicenna and others), 11 Scottish, 12 Danish, 16 Swiss, 17 Austrians, 22 Greek, 36 American, 40 Dutch, 77 English, 78 Italian, 127 French and 240 Germans, making a total of seven hundred and thirty-six. The citizenship of many of the men studied has been hard to determine on account of the migration of teachers from country to country, which in the seventeenth, eighteenth and nineteenth centuries has been very common; but the above is a fair representation of the proper distribution of the men who have developed anatomy.

More than twenty-five professions are represented by the men who have been given a place in the list. In attempting to decide the position of a man in the following scheme it is not always easy, on account of the varied interests of some of them, to place them properly. Should Albrecht von Haller, for instance, be regarded as an anato-

mist, philosopher, poet, physiologist, botanist, or as an administrator, since he attained some eminence in all of these lines? Should Emanuel Swedenborg be classed as a philosopher, anatomist, geologist, civil engineer or theologian? In the present scheme Albrecht von Haller is arbitrarily regarded as an anatomist, although a very large share of his work was physiological. Swedenborg is regarded as a philosopher, for as such he is usually classed, although his anatomical writings were of a high type. Descartes is likewise regarded as a philosopher, although he might with justice be called a mathematician or anatomist. The subdivisions of histology and embryology are necessary since a few men specialized strictly in these branches of anatomical work, and they are known for their contributions to these subjects; such for instance as Balfour's noted studies in embryology and Corti's in histology.

The following list will show in a general way the numerical distribution of the men in various professions: 1 jurist, (Johannes Peyligk, who in 1499 published in Leipzig his "Philosophie Naturalis," which contains ten figures of separate organs of the body), 1 statistician, (Francis Galton), 1 beadle or exciseman (Leeuwenhoeck,[29] who for thirty-nine years worked as a subordinate customs officer or beadle at a salary equal to $125 per year. In spite of this meager income he contributed 375 papers to the Royal Society of London and 17 to the Academy of Science in Paris, besides making all of his microscopes), 1 pope (Innocent XII, who, working under the direction of Lancisi (1654–1720), is said to have been one of the first to observe, under the microscope, the circulation of blood in the capillaries); 1 prior, 1 journalist, 1 theologian (Caspar Bartholin, the founder of a professorial dynasty in the University of Copenhagen whose members taught in

[29] It should be noted that the following account is that of Sir Benjamin Ward Richardson, "Disciples of Æsculapius," Vol. 1, p. 111. Garrison, however, says in his "Introduction to the History of Medicine," p. 185, that Leeuwenhoeck was "an inheritor of well-to-do brewers (and) led an easy-going life."

the university for one hundred and thirty-five years); 1 lawyer (Michel Alberti who contributed nearly 300 separate works on several phases of human knowledge); 1 bibliographer (Mangetus, "Bibliotheca Anatomica"), 1 clergyman (Wm. Buckland), 2 monks (of whom one was Gregor Mendel and the other Michael Servetus, a Spanish monk, the discoverer of the pulmonary circulation, which he published in 1553, seventy-five years before the appearance of Harvey's great work on the motion of the heart and the circulation of the blood); 3 physicists (Helmholtz and others), 5 poets (Mark Akenside, who wrote his inaugural dissertation on the fetus; Goethe, who is widely known for his papers in comparative anatomy, and for his homology of the intermaxillary bone of men and mammals; Lucius François Anderlini, a surgeon of Saint-Angelo, in the duchy of Urbino, who published in 1739 a poetical work "The Anatomist in Parnassus, or a Compendium of the Parts of the Human Body arranged in Verse"; Scipion Abeille, a military surgeon in Flanders who wrote, in 1689, a poetical anatomy on the parts of the head and neck; Albrecht von Haller was a poet of note, and many other men, interested in anatomical subjects, have been poetically inclined), 8 artists (Albrecht Dürer is well known for a work on human proportions which is of value from an anthropological standpoint; Michelangelo, working with Realdo Colombo (1494–1559), became deeply versed in human anatomy; Leonardo da Vinci, about 1510, completed a wonderful series of anatomical sketches, based on his own dissections); 5 ophthalmologists, 5 anthropologists (Blumenbach (1752–1840) was the founder of this science); 5 comparative anatomists, 6 embryologists, 7 pathologists, 9 histologists, 8 botanists, 14 paleontologists, 17 philosophers (Aristotle, Descartes, Swedenborg, etc.), 24 physiologists, 52 zoologists, 69 surgeons, 175 physicians, and 255 professional anatomists who devoted most of their attention to the teaching of anatomy.

It has not been possible to determine accurately the

profession of many of the men included in the list, and for this reason many whose names are in the list are not classified here. For instance, Petrus d'Abano, who published in 1496 the first illustrations of the abdominal muscles, was either a physician or a professional philosopher, and probably the former, since Locy says the illustrations seem to have been based on a dissection. And there is a story concerning the large fees charged by Abano, which indicates that his profession may have been medicine, although his intellectual interests were chiefly philosophical. Bartholomæus Anglicus was probably a physician or a publisher. At any rate he published in 1485 one of the first printed illustrations (a wood cut) of any anatomical interest. Many surgeons have contributed to anatomy, and were really at the same time teachers of anatomy, such as Nicolas Ivanovitch Pirogoff, who wrote an enormous cross-section anatomy in five volumes, published in 1852; notwithstanding which he is classed in the list as a surgeon, since his anatomical teaching appears to have been incidental to his surgery.

The social status of the men who have developed anatomical knowledge has been difficult to determine because of scant biographical data. There is sufficient, however, to indicate that contributors to anatomical knowledge have been recruited from a wide range of social conditions. Some of them, and often the brightest, have lived in poverty. Others have been representatives of a much higher social class. It may safely be said that the study of biological matters has attracted attention of no special class, but that interest has been scattered. It may be said that the great majority of men who have developed anatomical knowledge have been men of moderate attainments, belonging to an average rank in the social scale. The above statements must be modified by the conditions under which the men lived and the age in which they lived. During the early centuries of the Christian Era living conditions in general were not so wholesome[30] as they have since become.

[30] Hirsch, August, "Handbook of Geographical and Historical Pathology," 3 vols.

Precocity and productiveness have gone hand in hand among the few anatomists who have exhibited these interesting traits; yet it is only fair to state that productivity has not been dependent on precocity. Such men as Bichat, Balfour, Haller, Vesalius, Johannes Mueller, Bernard Siegfried Albinus, Pollard, Sir Charles Bell, are rather unusual examples of precocity.

It may be of interest to give a few detailed accounts of some of these men. During the short period of seven years, beginning his career at the age of twenty-three, which Marie François Xavier Bichat (known as the father of histology) devoted to his scientific studies, he came to be recognized as one of the foremost biologists of all time. He exhibited unusual talents for prolonged and intense application to the pursuit of his favorite science. Besides editing the surgical writings of his teacher, Pierre Joseph Desault, in three volumes, he is himself the author of three separate works, any one of which would have secured him fame. Bichat's claim to recognition as a great biologist lies in his division, in 1800, of the tissues of the body into twenty-one non-microscopic varieties.

Francis Maitland Balfour ended his brief career at the same age as did Bichat, thirty-one; but during the few years he devoted to his favorite study of embryology he laid a secure foundation for lasting fame. Especially in his monograph on the development of the elasmobranch fishes and in his comparative embryology, he exhibited a broad grasp of the subject which has seldom been equalled in the same field of learning.

Andreas Vesalius, the great Flemish anatomist, descended from a family of learned physicians, began his study of anatomy at the age of fourteen with Dubois in Paris, and at the age of twenty-two was called to Padua to give public demonstrations in anatomy. His large work on human anatomy, "De corporis humani Fabrica," which earned him the title of the founder of modern systematic anatomy, was published when he was thirty. Although he lived for twenty years after its appearance he

did little or nothing to develop anatomy save to issue successive editions of his "Fabrica." Three years before his death there appeared from the press at Madrid his edition of Fallopio's anatomy.

Albrecht von Haller, Swiss anatomist, physiologist, poet, botanist and administrator, deserves to be regarded as the most precocious and one of the most productive of all the men who have contributed to the advancement of anatomy. At the early age of eight he is said to have compiled a biographical index of over 2,000 eminent men and women. This prodigious activity he continued for the next sixty years, and it is stated that he conducted a monthly scientific journal to which he himself contributed 12.000 articles on nearly every phase of human knowledge. Nor were his contributions superficial, for Sir William Turner says that his anatomical descriptions and his beautiful and accurate figures were the most valuable which had appeared up to that time (1746–51). A list of his medical writings alone fills eleven octavo pages of closely printed type. Late in life he returned to Berne from Göttingen, where from 1736–1753 he had held the position as professor of anatomy, physiology, surgery and botany, to engage in his native land in municipal administration.

Johannes Mueller, who, in the first half of the last century, became famed as an anatomist, zoologist, and physiologist, became, at the age of twenty-five, professor extraordinary of physiology at the University of Bonn. He began an early career of prodigious activity, which he continued for thirty-three years.

Avicenna at the age of seventeen was regarded as an excellent physician. At twenty-one he was the author of several treatises. He was called "The Prince of Arabian physicians" by his contemporaries.

Bernhard Siegfried Albinus (1697–1770), for fifty years a teacher at the University of Leyden, was called to the University at the age of twenty-one, from Paris, whither he had gone on the advice of his father, to study

medicine and especially anatomy with Winslow and Sénac. He had hoped to spend some years in Paris, but after six months, on the retirement of Rau from the professorship of medicine, anatomy and surgery at Leyden, Albinus was called, at the suggestion of Boerhaave, to take charge of the anatomy. Shortly after reaching Holland the University of Leyden gave Albinus his doctorate of medicine without either examination or thesis. His inaugural address "Oratio inauguralis de anatome comparata" clearly showed the master mind. At Leyden, Albinus gave a new direction to the study of anatomy which had lain dormant since the appearance of Vesalius's "De Corporis Humani Fabrica" (1543). He brought to greater perfection the art of anatomical illustrating, which had not progressed since Vesalius, and especially in his "Historia musculorum hominis, Leyden, 1734, in-4°", on which his fame as an anatomist rests. This magnificent work was twice reprinted and translated into French by Pierre Tarin in 1753. Albinus published also other valuable works and left a marked impression on his subject.

It would appear, from the above study, that the sources of anatomical literature are to be found in the writings of the men who have developed the subject in the various countries mentioned. The literature of anatomy has now attained sufficient dignity to warrant the preparation of a "Source Book," which would be very useful. Africa, aside from the Grecian incursion in the early centuries of the Christian era which resulted in the Alexandrian school, has produced no men of attainments in anatomy. South America has one man to its credit. Mexico and China have none. The literature of the rest of the world has radiated out from those European countries which have fostered our modern civilization. The outlook for an excellent type of anatomical literature in the future is better than it has ever been and the student who attempts to work in the field of the history of this literature will find himself among interesting and delightful surroundings.

THE CASE OF TRICHOMONAS[1]

DR. PHILIP HADLEY

THE great group of flagellated protozoa has, within the past two decades, afforded a wealth of interest for those concerned with pathogenic protozoology; and only in slightly lesser degree for those concerned with taxonomic problems involving these highly interesting microorganisms. The field of trypanosome research has, in itself, afforded much new data on morphology and on complicated life histories; and has been the chief center of interest for many years.

But there exists another group of the flagellated protozoa, represented by some of the commonest forms encountered in the intestinal tract of man and the lower animals, whose frequency of occurrence, simplicity of organization and freedom from imputations of possessing pathogenic powers, have enabled them to go their way, for the most part unmolested by the protozoologist. If the protozoan would escape the inquiring gaze of the researcher he must be self-effacing; he must lead a quiet life of seclusion, free from those public manifestations of unrest and mob movement which are sure to bring him, sooner or later, before the bar of investigation, whereupon his whole life is laid bare.

Trichomonas was such a quiet law-abiding protozoan before the trouble began, before he was detected in instigating internal revolutions which bid fair to annihilate the turkey-raising industry of the country. The circumstantial evidence which has been brought forward against him has served to reveal many aspects of the life history of *Trichomonas* with which we were not previously acquainted; to disclose his participation in activities for which he was previously regarded as scarcely capable, and to demonstrate the existence of certain family resemblances to some of his companions in mischief who have long been recognized as trouble-makers in the cell organizations of many animals.

[1] Contribution No. 231 from the Agricultural Experiment Station of the Rhode Island State College, Kingston.

Trichomonas is found living in the intestinal contents of nearly all animals and has, since its discovery by Donné in 1837, appeared under many different names. It is a small organism, built on an oval or pear-shaped plan, and measuring in the adult trophozoite stage, about 10 μ in length by 5 to 6 μ in breadth. The youngest free-swimming stages are much smaller, about 5 μ in length; and sometimes trophozoites are encountered that measure 12 or 13 μ. Although usually of an elongate oval or pear shape, the morphology of the trophozoites is highly variable, and triangular or crescentic forms are frequently encountered, especially among the young. The anterior end is usually blunt, while the posterior end is frequently drawn out into a point.

If one adds to the salt solution in which these flagellates are being examined a little albumen or glycerin, to lessen the rapid swimming of the organism, some of the details of structure can be made out. The body plasm shows a greenish tint, and the nucleus, which is situated anteriorly, appears pinkish. In fresh preparations, one of the most obvious features is the axostyle, a short bristle-like structure which projects outward somewhere in the posterior quarter of the body, and which is seen, upon careful focusing, to extend into the body of the flagellate, running anteriorly to terminate somewhere in the vicinity of the nucleus (Figs. 1, 2). Inside the body the axostyle appears homogeneous in structure and bandlike.

Next to the axostyle, the most obvious feature is the vibratory or undulatory membrane which extends like a curved fin down the dorsal side of the flagellate body (Fig. 1). It is shallow at the beginning and at the end, but midway of its length it may have a depth of 2 to 3 μ. Over this membrane may be seen to travel at 3 to 4 μ intervals, waves of motion from the anterior toward the posterior end of the body. If one follows closely the course of this membrane, it is found to have its origin in a granule, or in a group of small granules, located in front of the nucleus at the most anterior part of the animal, and known as the blepharoplast-complex. The granules

are very small, measuring not more than 0.5 to 1.0 μ, and stain deeply with the chromatin stains. Their function is at the present time only a matter of speculation. Tracing the dorsal membrane posteriorly, it is found to extend to the extreme end of the body, where it narrows and is continued in the form of a terminal flagellum ("Schleppgeissel") which has a length ordinarily about equal to that of the body.

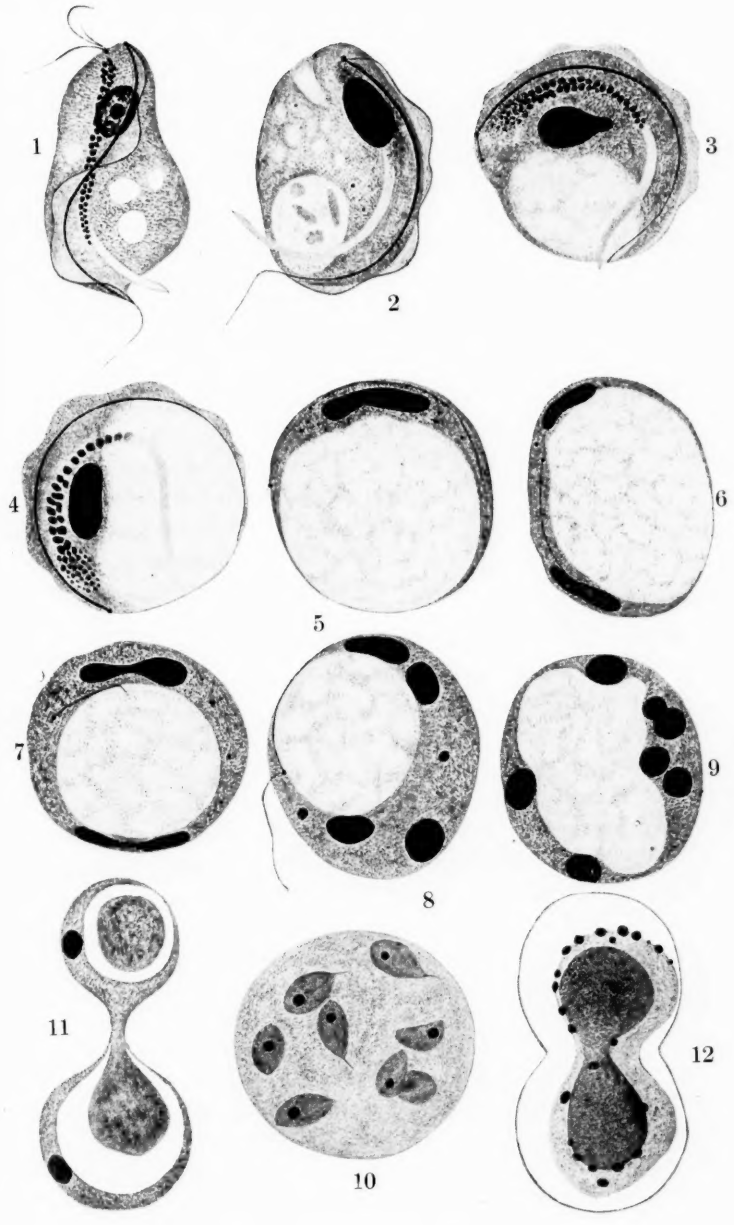
From the anterior end of the flagellate extend three more flagella (Fig. 1). These may be even longer than the body of the flagellate itself, and beat downward, as indicated by the arrow in Fig. 1. It frequently appears as if two of these three flagella were united in a common stalk at their base, so that they beat together, while the third flagellum beats independently. The origin of these three flagella is difficult to make out, but in many cases they appear to arise from one of the granules of the blepharoplast-complex, and usually not from the granule which is the origin of the undulatory membrane.

The only other structures which can be seen well in fresh preparations are the mouth or cytostome and the food vacuole. The cytostome is a horn-shaped opening which extends into the body on the ventral side, and just behind the nucleus (Fig. 2). It may be bordered by cilia. The beat of the anterior flagella is in such a direction that currents of fluid containing the bacteria which serve as the chief food for the flagellates, are driven into the mouth opening. Posterior to the nucleus, usually in about the middle of the cell body lies an oval space, the food vacuole (Fig. 2). It may sometimes be represented by a group of smaller vacuoles which coalesce to form a single cavity. In these vacuoles are usually present bacteria and cocci undergoing digestion. The structures mentioned above can be seen well in unstained organisms, but there are others which appear to advantage only upon staining. In preparations stained by the Heidenhain iron-hematoxylin method (wet process) the most noteworthy of the remaining structures is the chromatic line. This is a heavily-staining band which extends like the arc

of a circle from the blepharoplast to the point where the undulating membrane terminates. It thus follows closely, in the body plasm, the trend of the membrane, and is regarded as representing a kind of supporting structure. The chromatin line is heavier in its mid part and tapers at each end. As will be pointed out later, when in the process of spore formation, the trophozoites round off, the chromatin line becomes bent into a hoop, so that its extremities come very near to meeting (Fig. 4).

Another structure which appears with distinctness in stained preparations is the line of chromatic blocks (Figs. 3, 4). These peculiar bodies appear as a single or double row, or as a somewhat irregular line, of deeply staining granules extending from the region of the blepharoplast backward through the plasm to end somewhere in the posterior quarter of the cell. The anterior portion is likely to be thicker and sometimes may partially obscure the nucleus. The curve followed by the line of blocks is about parallel to that of the chromatic line and the two are seldom far distant from one another.

It is interesting to observe in connection with all of these structures that, in their arrangement, they produce in the flagellate organism a more or less perfect bilateral symmetry. The normal swimming position of the trophozoite is with the undulatory membrane above. Directly below this extends the chromatic line and below the chromatic line is the "line of blocks." The cytostome is in the midline and somewhat ventral. The blepharoplast is in the midline except in some of the stages of division. The food vacuoles occupy a variable position, but are usually grouped near the middle of the posterior body and caudad of the chromatic line. Sometimes it appears as if the line of blocks and the axostyle passed through the food vacuoles. The axostyle projects from the cell body in the midline although not necessarily at the most posterior part of the body. This symmetry is easily seen when the organisms are observed swimming freely in a favorable medium. Owing to the fact that the dorso-ventral diameter is greater than the transverse diameter, most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of the flagellates when stained on the slide present a lateral aspect as shown in Fig. 2, since they fall over on to their side in the drying out of the film.

But the appearance of the flagellate as described above does not endure for very long, simply because the trophozoite stage itself does not endure. The development of the trophozoite marks the period of youth, and when the organism has sufficiently fed on bacteria and cocci, and obtained a sufficient amount of reserve food, it passes on either into division or into a form of autogamous reproduction by which the flagellate population is increased at a rapid rate.

In the case of division, the process seems to be for the most part longitudinal. The first indication of it is to be seen in the blepharoplast-complex and in the nucleus. From each new blepharoplast there appears to grow out a new chromatic line, extending more or less parallel to the old line. From these new lines the new undulating membranes appear to arise. The writer has not been able to observe the division-stages of the flagella, although stages have been seen in which new flagella are present in connection with each new blepharoplast. Neither has it been possible to follow the changes in the axostyle. As to the chromatic blocks, these also seem to disappear and are probably formed anew in the daughter cells.

But reproduction by division, though occurring commonly in the intestinal content, is probably not the chief method of reproduction. At all times, though at some times more markedly than at others, the flagellates enter into a course of autogamous reproduction in which several daughter cells are formed out of a single mother cell. This interesting process can be followed in considerable detail by means of suitably stained smear preparations.

The first step in this process is the "rounding-off" of the previously elongate or crescentic trophozoite after it has reached maturity. If the body-form was crescentic there occurs a filling-out of the concave surface so that at first a full oval shape is produced; later the organism becomes spherical. This rounding-off process, which is

usually accompanied by some increase in size, is marked by important changes in the structures alluded to above. These may be considered in some detail first with reference to the external features.

Perhaps the most noteworthy change, aside from the assumption of a spherical shape, involves the chromatic line. This gives the appearance of lengthening until it forms a hoop almost completely encircling the organism (Fig. 4). It is common to see the ends of the line occupying positions less than 45 degrees apart as measured on the circumference of the spherical flagellate. At the same time the flagella have been lost and the undulatory membrane has decreased in size, though it follows approximately that part of the circumference corresponding to the chromatic line. Of course its functioning has been proportionately reduced and although its undulatory motion may continue, this movement fails to cause progressive movement of the flagellate, but brings about a slow rotation of the organism in the same position. Occasionally this movement may be assisted by a single anterior flagellum or a remnant of one which remains after the others have disappeared. In the final stage all trace of the cytostome is lost, and in fresh preparations the organism appears as a ball of fairly homogeneous fluid, surrounded by a granular cytoplasm containing the nucleus (Fig. 5).

But more interesting are the changes that have been occurring in the internal structures, as revealed by stained preparations. The alterations in the food vacuole are possibly the most significant. In the trophozoite stage the vacuole was made up of one or more spaces representing probably not more than one eighth to one tenth of the organism (Fig. 2). As the rounding-off process proceeds, the vacuole increases in size until it occupies the greater part of the ventral portion of the flagellate (Fig. 4). It begins to crowd the cytoplasm against the dorsal wall, and in this area lies the nucleus, which, as a result of pressure, becomes somewhat flattened. At the same time the "line of blocks" and the axostyle, which gives the

appearance of passing through the food vacuole, begin to degenerate and eventually both disappear. The chromatic line endures for a longer period, however, and remnants of it may be seen for some time after the "line of blocks," axostyle and undulatory membrane have vanished. The blepharoplast also can be detected as long as the remnants of the chromatic line are visible (Figs. 6, 7). This increase in the size of the food vacuole seems to be due, partly at least, to the taking-in of fluid, since while this process is occurring the flagellate is increasing in size and becoming more plastic in the constitution of its protoplasm.

The food vacuole has now increased in size to represent the greater part of the flagellate cell and is surrounded by a crescentic ring or layer of cytoplasm seemingly much reduced in amount (Fig. 5). From this time on the most important changes concern the nucleus. This is now flattened or sometimes flask-shaped, and soon divides into two equal portions which travel through the region of cytoplasm to take positions at opposite sides of the ball of reserve substance (food vacuole). Here each experiences a further division resulting in the production of four daughter nuclei (Figs. 7, 8). These apparently may divide again until either eight or sixteen daughter nuclei are formed occupying positions about the periphery of the cell. Frequently smaller portions of nuclear substance are to be seen in the cytoplasm following the first nuclear division and it is probable that these represent reduction bodies (Fig. 8), although the writer has not observed them in the course of formation. About the daughter nuclei there seems to gather by slow degrees a layer of cytoplasm and eventually they break out of their peripheral ring of maternal cytoplasm to enter the ball of reserve substance occupying the center of the cell (Fig. 10). This is gradually consumed by the young organisms which slowly take on an elongated shape. During this time the cyst wall which had formed about the mother cell has been weakening and finally the young organisms break out of the mother cell and appear as the youngest

trophozoites measuring from 4 to 5 μ in length and about 3 μ in breadth, equipped with anterior flagella at least, and possessing a relatively large nucleus and minute blepharoplast. The other organelles characteristic of the mature trophozoites appear to develop by degrees as the trophozoite increases in size.

These, then, are the two chief methods of reproduction. Ordinarily the course is very simple, but from a study of both fresh and stained material it is clear that several complicating factors may enter. For instance there is evidence that conjugation may occur, not only between two individuals but perhaps between three or four. This process is aided by the extrusion of a viscid membrane by those organisms that have rounded-off. This naturally helps to cause the individuals to adhere together. After conjugation this viscid membrane appears to harden into a protective cyst wall. Usually the size of the single cyst is about 10 to 12 μ, but in the "fused" or conjugated forms the diameter may reach 20 to 30 μ as seen in fresh preparations. It is also clear that the "double" and "triple" cysts sometimes seen may represent a division of the original cyst, whereupon each daughter cyst continues independently the production of daughter cells by the usual method, described above.

Reproducing by the methods described above, *Trichomonas* ordinarily lives in the intestinal tract and causes no recognizable injury to the host. It has never been regarded as other than a harmless commensal. Recent studies[2] have demonstrated, however, that, upon occasion, this flagellate may depart from its usual mode of life, may penetrate the tissues of its host and cause fatal lesions, not only in the walls of the intestinal tract, but in the liver as well. It is especially this assumption of a pathogenic rôle, this sudden adaptation to a new manner of life, together with the morphological changes that accompany it, that constitute perhaps the most interesting phase of the life of *Trichomonas*. First, how does it happen that the flagellate gets started on its tissue-despoiling career?

[2] Rhode Island Agricultural Experiment Station, Bul. 166, 1916.

What is the first stimulus that creates out of a commonly law-abiding protozoan, an invader that has no equal among protozoan forms in the rapidity and completeness with which it carries on its ravages in the intestinal tissues?

This is a difficult question, and one which can not be answered with any degree of finality at the present time. The facts of the matter are these: The manifestation of the disease, as it appears for instance in the so-called blackhead of turkeys, is invariably preceded by a diarrheal condition in which the flagellates appear in increasing numbers as the course of the disease advances. Finally they appear, not only in the liquid cecal content, but in the very depths of the cecal tubules or crypts; and finally in the tissues behind the epithelial wall. From this position, by a process of autogamous reproduction, the invasion of the mucosa, submucosa, muscularis mucosæ and even the muscular layers, goes on rapidly; and eventually the whole cecal wall is crowded with the parasites. Secondary bacterial infections may intervene and the results are almost invariably fatal. The question now arises: Are these countless flagellates, present in the liquid cecal contents at the beginning of the attack, the *cause* or the *result* of the diarrheal condition? Clinical evidence, which can not now be presented in detail, seems to indicate that the latter circumstance is the actuality: that the diarrhea is the primary condition and the increase in the number of parasites the secondary. To explain the "first cause" of the disease, then, one must explain the cause of the diarrheal condition; and this, of course, is likely to prove in itself, a complex problem, but seems to lead back to certain circumstances related to the nature of the food materials and their assimilation, lying outside the province of the present paper.

For a long time it was not clear how, after their rapid multiplication in the intestinal content, the parasites were able to penetrate the epithelial wall and reach the subepithelial tissues. Recent studies[3] have shown the rôle

[3] Rhode Island Agricultural Experiment Station, Bul. 168, November, 1916.

played by the goblet or chalice cells of the crypts of Lieberkühn in this respect. *Trichomonas,* after congregating in vast numbers in the fundi of the crypts, with a consequent bulging of their walls forces its way into the goblet cells. It is not deterred by the nucleus or the cell wall at the basement end, but throws the former out of place and breaks through the latter to assume a position beneath the epithelium of the crypt. The wall having been ruptured and an avenue created to the deeper tissues, other flagellates follow by the same path until many are present between the epithelium and the basement membrane. But *Trichomonas* does not halt here. It is now filled with the spirit of the invasion and quickly pushes through the basement membrane into the loose connective tissue of the mucosa. This tissue is speedily overrun by the advancing hosts, the barrier of the muscularis mucosæ is passed and the entire submucosa exposed to the ravages of the parasite.

It is here that we recognize *Trichomonas* in a new rôle. Having experienced its first taste of blood its whole nature is changed; it becomes another animal, raging through the tissues and impeded by no protective action that the host organism is able to muster to the defense. Here then we must recognize *Trichomonas* as a cell parasite, an organism that has the power to actively invade living cells and to bring about their destruction. One may remark that the type of cell invaded is highly specialized type, and one that, by its nature, is more or less open to invasion. But the fact remains that *host cells are invaded,* and actively invaded; and in this circumstance we can detect, in the behavior of *Trichomonas,* a foreshadowing of those cell-invading activities regarded as characteristic of the sporozoa.

But of course the host-organism must put up some defense, and sometimes a very vigorous defense is offered, chiefly by means of its batteries of endothelial and other phagocytic cells. These come out in numbers to meet the invaders and as a result many of the flagellates are engulfed, either by single endothelial cells or in giant cells.

But the curious part of this circumstance is that the engulfing of the parasites seems to be of slight avail in retarding the invasion; and, in many instances without appreciable detrimental effect upon the parasites engulfed. From observations on the staining reactions and on the morphological features of the ingested parasites there is good evidence that *Trichomonas* is not disintegrated by the process; and much less killed outright. It shows a marked resistance to the plasm of the endothelial cells, within which it frequently appears that development may proceed, and from which a new generation of flagellates may break out to continue the course of infection. This would imply that the parasites, once engulfed, are able to make use of the plasm of the endothelial cell as food. And some evidence actually seems to support the view that the parasites fare better in the endothelial cells than they do without. In any region of invaded tissue the majority of the organisms are present within the engulfing cells. If these views should prove valid it must be admitted that a curious situation is produced: the parasites, to survive, must be ingested by the defensive cells, while these phagocytic agents in carrying out their normal defensive function, are favoring the growth and activity of the invaders. Of course the residence of *Trichomonas* within the endothelial cells is purely a passive cell-parasitism, although the penetration of the goblet cells is an act of active cell-parasitism. But when we regard both together, the matter is of considerable interest in its bearing upon the origin of the sporozoa, cell parasites most exclusively. From such elementary invasive power and from such primitive toleration of unfavorable host-cell influences as we see in *Trichomonas*, it is easy to imagine how the most effective stages of sporozoan parasitism may have evolved. It is a beginning of that marked adaptability of form and of physiological organization which lies at the base of all pure parasitism as it occurs in the higher orders of the protozoa.

Another noteworthy feature in the life of *Trichomonas*, and one which again serves to connect the organism with

the accepted type of sporozoan parasitism deals with the manner of obtaining its food. It has already been pointed out that when the trophozoites are developing in the intestinal contents they ingest large numbers of bacteria; whether, at this time, osmosis plays any part in cell nutrition is a question. When *Trichomonas* has entered the deeper tissues, however, the situation is different, since there are ordinarily few bacteria in these regions. Here it seems that nutrition by osmosis must play an important rôle in supporting the life of the rapidly multiplying organisms. It thus appears that *Trichomonas* is sufficiently adaptive to new conditions of existence in the tissues to substitute an osmotic method of nutrition for the ingestive. This nutrition by osmosis it will be at once recognized is one of the characteristic features of the sporozoa, and here again is to be seen a link connecting these two protozoan types.

But there is another point of interest involved in this change in the manner of nutrition when *Trichomonas* enters upon its tissue despoiling career, and this concerns the influence of the manner of nutrition upon some of the morphological features of the parasites.

In regarding the appearance of the flagellates preceding their invasion of the tissues, and after they have gained a foothold in the submucosa, a marked difference is to be observed. This has already been mentioned and may be so great as to deceive one into the belief that the parasites which are found in the intact crypts and which penetrate the epithelial wall, are not identical in nature with the organisms occurring in the deeper tissues. It is this difference which has led some writers to believe that we are dealing with two different protozoan forms. The difference lies primarily in the following circumstance: In the cecal content the flagellates are represented by two forms, the motile trophozoite and the encysted organism. In the case of the latter, one can usually observe clearly the large ball of reserve-substance, and the relatively large daughter nuclei. When developing in the tissues, on the other hand, although the motile forms can be recognized

without difficulty and although the sporulating forms, characterized by the presence of the daughter nuclei, are also observable, both of these are relatively uncommon, and the stage which shows the well rounded ball of reserve-substance ("Reservestoffsballen") is seldom met with. How can these phenomena be explained?

The writer has introduced this point in connection with the discussion of the methods of nutrition of the flagellates, simply because it seems possible that the morphological differences alluded to above are conditioned by the nature of the food supply. The writer has already traced the changes which the food vacuole of *Trichomonas* undergoes during the process of encystment. It was shown that there is a direct transformation from the food vacuole of the trophozoite, laden with bacteria and cocci, to the ball of reserve-substance which eventually crowds out the nucleus, chromatic line and line of blocks from the inner part of the cell and may possibly absorb the axostyle. Finally it comes to lie as a mass of varying size with respect to the cell, in the center, or slightly to the ventral side of the organism. Its staining qualities suggest a glycogen-like substance, and its density appears to vary with the stage of digestion of the food substances which are to serve the young daughter cells.

As stated above, this well defined reserve-substance mass is seldom observed, at least well developed, in the flagellates located deep in the tissues where the evidence favors a view of nutrition by osmosis. The question is therefore raised: Can it not be that the marked difference between the appearance of the flagellates in the tissues and in the cecal content is dependent directly upon the nature of the store of reserve food; and thus indirectly upon the manner of nutrition. This view is in agreement with the general observation that protozoa that subsist by osmosis seldom manifest either food vacuoles or definitely segregated bodies of reserve food substance.

In just what way the presence or absence of a ball of reserve-substance would explain all the differences observed in the parasites in and out of the tissues it is diffi-

cult to say. That its absence would determine a more homogeneous cytoplasm at all stages of growth is, of course, obvious, but its effect upon the cell structures such as chromatic line, blocks and axostyle, and upon the relative size and distribution of the daughter nuclei is still not clear. It can scarcely be wondered at, however, that such a radical change in the manner of nutrition of a parasite would be accompanied by alterations of some developmental significance.

Upon superficial observation it appears that, in a parasitism of this sort, when the organisms are driving ever deeper into the tissues, one of the essential features of complete parasitic activity is absent, namely, the ability to escape from the tissues and to secure a position by virtue of which the parasite can insure the possibility of reaching other hosts. Without this possibility provided for, no parasitism can be called complete. Although, in the case before us, many of the parasites are so buried in the tissues, a study of the trend of the infective process as a whole has revealed a means by which the organisms return to the cecal content after their invasive career has ended. This is by spreading downward and inward through the reticular tissue of the cores of the villi and pushing the epithelium off of the villus tips. Behind the epithelial wall at these points the parasites congregate in vast numbers until finally the epithelium breaks and liberates the flagellates into the cecal contents. That this process of escape from the tissues takes place only over certain areas of the intestinal wall is apparent; but the fact that it occurs at all is sufficient evidence to indicate that *Trichomonas* is not wholly lacking in this essential element of successful parasitism.

And finally we find in the case of *Trichomonas* one more lesson, and this is one for the etiologist, this being of course any one who concerns himself seriously with disease etiology. This important person, confronted with a disease of unknown cause, busily sets about to discover the germ; and having found the germ, he as busily engages himself in ascertaining means and measures

whereby the germ may be avoided by all susceptible folk. We are warned to avoid the places where the germ lurks, to boil our drinking water and to put cotton in our noses; and of course this has been of immense value in preventing infection in the case of many communicable diseases. But this conception of escaping the germ, a procedure still by force of habit widely applied, unfortunately does not work out successfully in all cases, simply because we have at last found that the germ is not always escapable. It may be right with us day and night; and whether we succumb to an eventual invasion depends not upon our side-stepping the organism, but upon our maintaining certain of the body defenses at the proper level of efficient working. The case of *Trichomonas* in its proper host is an instance. For twenty years (under other names) it has been consistently avoided and wholesomely feared by intelligent turkey raisers. Five hundred regulations more or less have been directed against it; and now we find that it is always there and always will be there. To keep it in an amicable state, to deter it from making destructive excursions into the tissues, all that is required is to maintain a normal and hygienic condition of the intestinal tract, whatever this may mean; this alone appears to be sufficient. Thus, although no other intestinal protozoan is able to exert, in a brief time, a greater destructive activity than *Trichomonas* when properly aroused, still we are far from justified in placing its name upon the blacklist of unqualifiedly pathogenic types which are, by both heredity and training, trouble-makers. On the other hand we can not continue to place this flagellate in that sainthood of parasites, the "harmless commensals," since, upon occasion, it may be far from harmless. *Trichomonas* must now be registered as a *facultative parasite,* which offers a wealth of interesting subject-matter for research covering several fields of biological study.

LINKAGE IN MAIZE: ALEURONE AND CHLOROPHYLL FACTORS[1]

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GENETIC linkages or correlations are beginning to confirm the modern chromosome conception of heredity. As the Mendelian analysis of a species reaches a point where the known genetic factors exceed the number of chromosome pairs, certain group relations between the factors should become evident. Comparatively few genetic linkages have been observed in plants, however, probably because the number of Mendelian factors that have been determined is relatively small compared with the number of chromosomes in most species.

Physiological or morphological correlations, on the other hand, are far more common. But in the present state of knowledge they are not classified as genetic and consequently can not be used as material for determining the relationship between any series of heritable factors.

An intensive, Mendelian study of maize is gradually revealing genetic correlations. Although more than thirty definite Mendelian factors have now been determined in this species, linkages are limited in number because of the relatively large number of chromosomes (at least nine pairs).

As early as 1906, Webber noted a general correlation in maize between color in the aleurone layer and in the stamens, glumes and silks. At that time, the genetic constitution of color in the kernels and other parts of the plant was unknown. Consequently the correlation was not analyzed on a factorial basis.

In 1911, Emerson described an apparent linkage between color of cob, pericarp, husks, silks, and anthers in

[1] Given before the Botanical Society of America at the annual meeting held in New York City, December 28, 1916. Paper No. 58. Department of Plant Breeding, Cornell University, Ithaca, N. Y.

maize. Further evidence (unpublished), however, has led Professor Emerson to prefer a simpler explanation for this phenomenon than linkage.

Collins and Kempton (1911) and Collins (1912) have reported a genetic correlation between aleurone color and endosperm texture. In this case one of the pairs of aleurone factors is apparently linked with one pair of endosperm factors (horny and waxy). The data indicate that there is a little less than twenty-five per cent. crossing over.

In a later paper, Collins (1916) describes five character pairs in maize that show apparent genetic correlation. But since these characters exhibit "blending" inheritance and have not been analyzed from any factorial standpoint, it becomes impossible to use these correlations in determining the group relations between the genetic factors concerned. Collins also notes a large number of physiological correlations in this article.

LINKAGE BETWEEN ALEURONE AND CHLOROPHYLL FACTORS

A definite linkage has been found in maize between one of the five pairs of aleurone factors (*Aa, Cc, Rr, Pp, Ii*)[2] and one pair of chlorophyll factors of which there are at least seven, as the writer has determined. Manifestly this is not a physiological correlation, for it is difficult to conceive of an anthocyanic pigment, limited to a single layer of cells in the grain, being caused by the same physiological factor that produces a plastid color, like chlorophyll, so widely distributed in the plant. Breeding evidence demonstrates that the correlation is genetic, as will be seen later.

The aleurone factor concerned is the *R* factor, which together with *C* and *A* is needed to produce any color in the aleurone cells of the corn grain (Emerson, 1917). To determine linkages with any of the five aleurone factors, it is obviously desirable to use material that is homo-

[2] Reported in a paper given before the Botanical Society of America at the annual meeting in New York City, December 28, 1916, by Professor R. A. Emerson. The paper is now in manscript form under the title, "A Fifth Pair of Factors for Aleurone Color in Maize."

zygous for as many of the non-linked factors as possible. Fortunately, the plants used in this experiment were homozygous for *A*, *P* and *i*, as will be shown later. This left only *C* and *R*, giving either 3:1 or 9:7 ratios of purple to colorless grains. The aleurone segregation on the ear proved to be exceptionally distinct because the factors *A* and *P* were homozygous.

The other factor involved in the linkage is concerned with chlorophyll development in the mature plant. It is one of at least seven factors necessary for the production of full, normal green color in maize, and it has been termed the *G* factor. Its allelomorph *g* produces a distinct yellow or golden color in the leaves and stalk of the mature corn plant. This color is comparable with that of the familiar golden-leaved shrubs. It has been described and its inheritance discussed by Emerson (1912) and Miles (1915). Suffice it to say that it is a simple recessive to normal green.

During the summer of 1914, a green plant heterozygous for *R* and *G* was pollinated by a golden plant that lacked aleurone color. The cross can best be described by the following factors:

$$\frac{GgRrCCAAPPii}{3472\ (11)} \times \frac{ggrrCcAAPPii}{3468\ (10)}.$$

Proof for the correctness of these formulæ will be given later.

A selfed ear of the female parent bore a 3:1 ratio (271:88) of purple to colorless grains, showing that only one aleurone factor was heterozygous. The male parent was likewise selfed and showed no aleurone color. On the ear of this cross there were 67 purple and 55 colorless grains, approximating a 1:1 ratio.

The purple seeds on this ear were planted separately from the colorless seeds. From the field counts of this planting, which are given in Table I, it is seen that the 1:1:1:1 ratio of independent inheritance is noticeably modified.

The observed numbers in the four classes give a ga-

metic ratio of 3.9:1, or 20.4 per cent. of crossovers. It is evident that the actual results agree very closely with the theoretical expectancy on a 4:1 basis of linkage. Indeed, the goodness of fit (Elderton, 1901 and Harris, 1912) is so perfect that $x^2 = .317$, giving a very high value for P.

TABLE I

SHOWING THE F_1 DISTRIBUTION FROM THE CROSS

$$\frac{GgRrCC}{3472\ (11)} \times \frac{ggrrCc}{3468\ (10)}$$

	From Purple Seeds		From Colorless Seeds	
	Green (*RG*)	Golden (*Rg*)	Green (*rG*)	Golden (*rg*)
Field counts	30	9	6	28
Observed, corrected for aleurone ratio[3]	28.1	8.4	6.5	30.0
Theoretical, on a 4:1 gametic ratio	29.2	7.3	7.3	29.2

From these results it may be said that, at gametogenesis, the factors of the female parent are so linked that the gametes *RG* and *rg* are produced about four times as often as the crossover gametes *Rg* and *rG*. Thus far, no evidence is available for demonstrating the linkage of *Rg* and *rG* ("repulsion" between *R* and *G*).

Additional evidence on the linkage was afforded when the female parent of the cross and four other plants, in which the *R* and *G* factors were heterozygous, were selfed. Each of these plants bore ears with 3:1 ratios in aleurone color (total, 1052 purple:339 colorless). The results appear in Table II.

Obviously this F_2 distribution does not resemble a 9:3:3:1 ratio of independent inheritance. The agreement between the actual results and the theoretical on a 4:1 gametic ratio, however, is close, *P* being .6733, which is considered a good fit. It may be concluded, therefore, that the factors in these five F_1 plants were so linked that *R* and *G* occurred together in one chromosome, while *r* and *g* were located in the homologous chromosome and

[3] This correction is applied to equalize the proportion of the plants from the purple and the colorless seeds. The proportion should be 1:1 in this case, but variation in the percentage of germination or in the number of purple and colorless seeds actually planted, often disturbs it. Such a correction is of course legitimate.

that crossing over occurred about twenty per cent. of the time.

TABLE II

SHOWING THE F_2 DISTRIBUTION WHEN F_1 PLANTS HETEROZYGOUS FOR *R* AND *G* WERE SELFED

Selfed Plants	From Purple Seeds		From Colorless Seeds	
	RG	*Rg*	*rG*	*rg*
3472 (11)	48	4	12	24
3347 (2) parent of above	35	3	8	6
3954 (1)	5	0	2	2
3954 (3)	10	0	2	3
3954 (11)	8	3	2	6
Total	106	10	26	41
Correction for aleurone ratio[4]	125	12	18	28
Theoretical, 4 : 1 basis	120.8	16.5	16.5	29.2

When plants that were heterozygous for *G* and for both *R* and *C* were selfed, the ratio between the four classes in the next generation appeared rather unusual. This was found to be due to the influence of the 9 : 7 ratio in aleurone color of each of the four plants that were selfed (total, 477 purple : 387 colorless). The data from such plants are arranged in Table III, which follows:

TABLE III

SHOWING F_2 DISTRIBUTION WHEN PLANTS HETEROZYGOUS FOR *G*, *R* AND *C* WERE SELFED

Plants Selfed	From Purple Seeds		From Colorless Seeds	
	Green	Golden	Green	Golden
3347 (1)	32	3	11	4
3347 (3)	79	6	51	31
3347 (4)	96	6	47	40
3477 (2)	38	3	19	11
Total	245	18	128	86
Theoretical 4 : 1 basis	236	32	122	87

There is considerable deviation from the theoretical expectancy in this case and the value for *P* is small ($P = .0808$). Nevertheless, these results, taken in con-

[4] The correction is applied because at planting time no attempt was made to plant three purple grains to one colorless grain.

junction with those in Tables I and II, accord with the idea of linkage between *R* and *G* on a 4:1 basis, especially since they deviate so widely from the 27:9:21:7 ratio of independent inheritance.

Another source of evidence on the genetic interrelations of the *R* and *G* factors was noted in a back cross in which the *I* factor for aleurone color was involved. No aleurone tests have been made, but evidence from related plants in pedigree cultures makes it seem reasonable that the following factors are concerned:

$$\frac{GgRrccAAPPIi}{3021\ (1)} \times \frac{ggrrCcAAPPii}{3018\ (10)}.$$

In this case, the ears of both parents showed no aleurone color. The ear from the cross gave a distinct segregation of purple and of colorless grains (36 purple:195 colorless), approximating the theoretical 1:7 ratio. Assuming the factors as given above, this aleurone ratio is reasonably close to the expected proportion, the numbers being relatively small. As a check upon this aleurone ratio, it might be mentioned that six of the F_1 plans from purple seed were selfed. Each one showed a 9:7 ratio of purple to colorless grains on the ear (total, 1441 purple:1088 colorless). Also three F_1 plants from colorless seed were selfed and the ears showed no aleurone color.

The field counts of the plants from purple and from colorless F_1 grains are classified in the following table:

TABLE IV

SHOWING THE F_1 DISTRIBUTION FROM THE CROSS

$$\frac{GgRrccAAPPIi}{3021\ (1)} \times \frac{ggrrCcAAPPii}{3018\ (10)}.$$

	From Purple Seeds		From Colorless Seeds	
	Green	Golden	Green	Golden
Observed	12.0	3.0	8.0	22.0
Correction for aleurone ratio	4.5	1.1	10.4	29.0
Theoretical, 4:1 basis	4.5	1.1	18.0	21.4

Here again, while the value for *P* is relatively small ($P = .1171$), it is noted that in general the observed re-

sults correspond with the theoretical when a linkage on a 4:1 basis is assumed. Considering only the classes from purple seeds (first two columns in Table IV), the agreement is perfect. These classes are not disturbed by the aleurone situation and consequently ought to show the gametic ratio directly.

Determination of the Aleurone Factor concerned in the Linkage

It has been shown from four independent sources that the *G* factor is linked with one of the five aleurone factors, which has been termed the *R* factor. It now becomes necessary to prove that it really is the *R* factor that is involved. From the aleurone ratios observed in this experiment, as well as from evidence from related plants, it is certain that the inhibiting factor *I* is lacking, with the exception of the last cross described (Table IV). It is also obvious that the *P* factor, which produces purple color when *C*, *R* and *A* are present, must be homozygous (*PP*) in both parents of the first cross since the color in the colored grains is a deep purple with no trace of red. This leaves only *C*, *R* and *A* to be considered.

In order to facilitate the presentation of the proof that the *R* factor is the one concerned, Table V has been prepared. In this table, the zygotic formulæ of the F_1 plants of the first back cross (see Table I) are listed and the plants that were tested are noted in the last column. The factors are merely assumed as here given.

TABLE V

Showing the Zygotic Formulæ of the F_1 Plants of the Cross 3472 (11) × 3468 (10)

Parental Formulæ: GgRrCCAA × ggrrCcAA

F_1 Zygotes	F_1 Aleurone Color	F_1 Plant Type	F_1 Plants Selfed (Pedigree No. 3954) Individual Plant Nos.
GgRrCCAA	Purple	Green	(1) (3) (11) (31) (33)
GgRrCcAA.....	"	"	(34)
ggRrCCAA.....	"	Golden	
ggRrCcAA.....	"	"	(32)
GgrrCCAA	Colorless	Green	(44)
GgrrCcAA.....	"	"	
ggrrCCAA	"	Golden	(21) (22) (29) (43) (45)
ggrrCcAA	"	"	

The fourth column shows the plant numbers of the F_1 plants, which were self-fertilized. In addition, plants (31), (32) and (34) were crossed with certain aleurone testers of known constitution provided by Professor R. A. Emerson.

All the grains on the ears of plants (21), (22), (29), (43), (45) and (44) were colorless. The aleurone counts on the ears from purple seed are arranegd in Table VI.

TABLE VI

SHOWING THE ALEURONE COUNTS ON SELFED EARS OF F_1 PLANTS FROM THE CROSS 3472 (11) × 3468 (10)

Ped. 3954 Plant No.	3 : 1 Ears		9 : 7 Ears		
	Purple	Colorless	Ped. 3954 Plant No.	Purple	Colorless
(1)	230	83	(32)	66	65
(3)	303	88	(34)	172	163
(11)	248	80			
(31)	180	45			
(33)	224	79			
Total	1,185	375	Total	238	228
Theoretical	1,170	390	Theoretical	262	204

These ratios indicate that plants (1), (3), (11), (31) and (33) are heterozygous for one aleurone factor only. Plants (32) and (34) also contain that factor but apparently they have in addition another aleurone factor, which likewise is heterozygous because the aleurone ratios, although they deviate somewhat from the 9 : 7 proportion, can not be classified as 3 : 1 or 27 : 37 ratios.

Only tests with plants of known aleurone formulæ will determine whether it is the *C*, *R*, or *A* factor that is linked with *G*. Such tests have been made by using the aleurone testers described by Emerson (1917). These aleurone testers possess colorless grains in which all the aleurone factors except *P* are homozygous. For example, the *R* tester has the formula *rrCCAAii*, the *C* tester the formula *RRccAAii*, and the *A* tester the formula *RRCCaaii*. In Table VII are presented the data involving the various tests for the aleurone factors. Plants (31), (32) and (34) possess all the three aleurone factors in question

(*R*, *C*, and *A*), as can be seen from their records in Table VI. Plant (41) is a golden type from colorless seed.

TABLE VII

SHOWING THE TESTS FOR THE PRESENCE OF ALEURONE FACTORS

Crosses with Aleurone Testers	Aleurone Color of F_1 Grains
1. *A* tester (*RRCCaa*) × 3954 (32)	450 purple
2. *C* tester (*RRccAA*) × 3954 (32)	72 " : 69 colorless
3. *R* tester (*rrCCAA*) × 3954 (32)	7 " : 11 "
4. 3954 (34) × *C* tester (*RRccAA*)	205 " : 217 "
5. 3954 (31) × *R* tester (*rrCCAA*)	212 " : 249 "
6. 3954 (41) (*rrCcAA*) × 3954 (31)	92 " : 101 "

The data in Table VII prove several things, namely:

1. That the factors *A*, *C* and *R* must be present in all the plants tested, because color was produced in some of the F_1 grains of each cross.

2. That it is the *R* factor that is linked with *G*. This is demonstrated in the fifth cross. Plant (31) was a green plant from purple seed bearing an ear with a 3:1 aleurone ratio, showing that it was heterozygous for the one aleurone factor that is involved in the linkage. The test clearly proves it to be the *R* factor, the aleurone ratio from the fifth cross approximating the theoretical 1:1 proportion.

3. That the assumptions made in Table V as to the genotypic formulæ of the F_1 plants are correct. This statement follows from a series of deductions. First, one aleurone factor of the three concerned must be homozygous (dominant) in all the plants, because the aleurone ratios observed permit only two heterozygous factors at the most (9:7 ratios). Since the *C* factor is heterozygous in plants (32) and (34), and the *R* factor heterozygous in plant (31), while *A* is homozygous in plant (32), it is obvious that only the *A* factor could be homozygous (dominant) in all the F_1 plants. Second, one of the remaining factors (*C* or *R*) must be homozygous (dominant) in some plants and heterozygous in others to account for the 3:1 and the 9:7 ratios, respectively. The second and fourth crosses in Table VII indicate that *C* is heterozygous in plants (32) and (34), whereas the fifth

cross, together with the 3:1 ratio of the selfed ear, shows that *C* is homozygous in plant (31). Third, the remaining factor, *R*, should occur only in a heterozygous or in a homozygous recessive condition to account for the 1:1 aleurone ratio on the F_1 ear of the original back cross. The fifth cross in Table VII proves that *R* is heterozygous in plant (31), and the sixth cross shows that in plant (41) this same factor is homozygous recessive.

From this series of interrelations it is seen that the hypothesis is verified in all cases and that it is the *R* factor for aleurone that is linked with *G*.

On the Question of Crossing Over in the Male and Female

An interesting observation regarding the question of crossing over in plants can be derived from some of the data presented. It has been shown that crossing over occurs in gametogenesis of the female (Table I). Does it take place in the formation of the male gametes as well? In certain animals, crossing over seems to be limited to one sex. It occurs only in the female of *Drosophila* (Morgan, 1915) and only in the male of the silkworm (Tanaka, 1914). Castle (1916) has noted that the phenomenon occurs in both sexes of the rat. Among plants, the studies with sweet peas and Primula indicate that crossing over is not restricted to one sex. Perhaps it is to be expected that, in the case of most plants, where the pistillate and staminate parts are borne on the same individual, there should be no difference in the genetic behavior of the two reproductive systems in this respect. Nevertheless, it is interesting to note the condition in the monecious corn plant.

From the data in Table II, it is possible to demonstrate that crossing over is found in both sexes of maize. In order to do this, the observed frequencies can be compared with the theoretical expectation when crossing over occurs in both sexes and when it takes place only in the female. Such a comparison is arranged in Table VIII. which follows:

TABLE VIII

A Comparison of the Results from Table II with the Theoretical Expectation when Crossing Over Occurs in Both Sexes and when it Occurs Only in the Female

	RG	*Rg*	*rG*	*rg*	*P* Goodness of Fit as Compared with Observed
Observed (from Table II).....	125	12	18	28	
Theoretical, when crossing over occurs in both sexes.....	120.8	16.5	16.5	29.2	.6733
Theoretical, when crossing over occurs only in the female.....	128.0	9.0	9.0	37.0	.0067

Clearly, the first theoretical expectation fits the case adequately, for with such a high value for *P* it is almost certain that the deviations of the observed results from the theoretical are due to errors of random sampling only. Consequently one is justified in saying that crossing over occurs in both male and female. This is especially true when the fit in the second case is so poor. In fact, the great difference between the two values for *P* makes it seem reasonable that the intensity of the linkage is equal in both male and female, although the high value for *P* in the first case suggests that directly.

Additional Linkages between Chlorophyll Factors and Aleurone

Preliminary tests indicate that the same chlorophyll factor, *G*, that is linked with *R*, is also concerned in a linkage with one of the other chlorophyll factors termed *L*. The latter has been found to be one of three factors, two of which have already been described by Miles (1915), involved in the production of chlorophyll in the seedling stage of maize.

Factors *G* and *L* seem to be linked although the data from three back crosses, in which the numbers are small, exhibit some variation in the percentage of crossing over. Details of this linkage will appear in a later paper, dealing with the inheritance of the three seedling chlorophyll factors.

Apparently, then, the factor pairs *Rr*, *Gg*, and *Ll* con-

stitute one factorial group in maize. It is to be expected that *Rr* and *Ll* should bear a definite relationship to one another. This has not yet been fully determined, although there are some indications of such a linkage, for aleurone color and chlorophyll development appear to be genetically related in different manner from that noted previously.

When purple seed of certain ears are planted separately from the colorless ones, the former give a distinct segregation of green and white seedlings, while the latter give rise not only to the green and white, but also to a constant proportion of yellow seedlings. The writer has determined that these yellow seedlings depend upon a definite genetic factor. Over two thousand seedlings have now been grown and not one yellow seedling has resulted from the purple grains. Discussion of this linkage will also be reserved for the later paper, or until the aleurone factor concerned has been identified. This may be the *R* factor showing its theoretical relationship to the *L* factor.

SUMMARY

1. Linkage between the *R* aleurone factor and the *G* factor for chlorophyll development shows approximately 20 per cent. crossovers.

2. Crossing over takes place in both male and female gametogenesis of the monecious maize plant.

3. Preliminary tests indicate that *G* is also linked with *L*, a seedling, chlorophyll factor. Consequently the factor pairs *Rr*, *Gg* and *Ll* constitute one factorial group in maize.

To Professor R. A. Emerson, of Cornell University, who so generously has shared his material for this investigation, the writer is deeply indebted and desires to express his sincere gratitude.

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SHORTER ARTICLES AND DISCUSSION

THE APPLICATION OF CORRELATION FORMULÆ TO THE PROBLEM OF VARIETAL DIFFERENCES IN DISEASE RESISTANCE: DATA FROM THE VERMONT EXPERIMENTS WITH POTATOES

THE ultimate practical object of any study of disease resistance in a series of varieties is the selection for future cultivation of the few which are least susceptible. In practise a relatively large series of varieties or strains is taken into cultivation for preliminary study. The size of the cultures of the individual strains must, on a given area, be inversely proportional to their number. Since the individual cultures are necessarily small, it is impossible to assert from the results of a single test that the observed differences between the strains really represent varietal differences in disease resistance. They may be due merely to inadequately large cultures or to imperfectly controlled experimental conditions. It is therefore necessary to repeat the experiment another year or in a different locality in order to determine whether the observed differences are really persistent, and so characteristic of the strain, or whether they are due to transient conditions only. The problem is then purely and simply one of correlation. This is obviously true whether one chooses to avail himself of the advantages of the statistical formulæ or not. If the correlation between disease incidence in cultures of the series of varieties grown in different years, or places, be zero, the varieties show no permanent differentiation in disease resistance. If the correlation has a significant positive value it indicates at once that there are really inherent varietal differences in disease resistance. The numerical magnitude of the correlation indicates something of the extent of this differentiation. If the correlation be low, the prospect of isolating varieties sensibly more resistant that the average will be slight. If the correlation be high, it should be relatively easy to secure highly resistant strains.

Since the correlation method seems to have considerable value in the analysis of data of this kind, I have thought it might be of service to geneticists and plant pathologists to illustrate it by the constants which I have found it necessary to deduce for another purpose from the published records of the series of experiments on disease resistance in varieties of potatoes carried on during

[1] Stuart, W., "Disease Resistance in Potatoes." *Bull. Vt. Agr. Exp. Sta.*, 179, 1914.

the past several years at the Vermont Agricultural Experiment Station.

In a recent bulletin Stuart[1] summarizes the data obtained during five years' observations on percentage infection by early blight (*Alternaria solani*). In his Table I he gives the estimated percentage infection in a series of varieties during the period. Since during a portion of the experiment all the varieties were not considered, I have calculated the correlations in two groups. In one case $N = 149$, in the other $N = 50$. The smaller group comprises only varieties also included in the larger. The correlations, calculated by the usual product moment method[2] without grouping, appear in the accompanying table.[3]

CORRELATION FOR VINE RESISTANCE TO EARLY BLIGHT IN TWO YEARS

Years Compared	Series of 149 Varieties	Series of 50 Varieties
1905–1906	$+ .055 \pm .055$	$- .056 \pm .095$
1905–1907	$+ .438 \pm .045$	$+ .420 \pm .079$
1905–1908	———	$- .021 \pm .095$
1906–1907	$+ .042 \pm .055$	$+ .226 \pm .091$
1906–1908	———	$+ .323 \pm .085$
1907–1908	———	$+ .082 \pm .095$

Only 2 of the 9 constants are negative; these are insignificant in comparison with their probable errors. All the constants which have substantial values and are materially larger than their probable errors are positive in sign. The average of the two negative constants is $-.038$, of the seven positive coefficients $+.227$, and of all the (unweighted) values $+.168$. Thus there is clearly a measurable differentiation of the varieties in respect to susceptibility to *Alternaria*.

The values are, however, exceedingly variable, ranging as they do from $-.056$ to $+.438$. The great variation in the actual constants I am inclined to attribute to (*a*) the difficulty of estimating the percentage of infection, (*b*) the unavoidable experimental errors associated with relatively small cultures, and (*c*) the wide variation in average percentage infection from year to year. Both (*a*) and (*b*) are factors which tend to render the actually recorded percentages somewhat erroneous as measures of the real susceptibility of the variety, and tend in consequence to dilute the strength of the correlation. With respect to the third

[2] AMER. NAT., 44: 693–699, 1910.

[3] The chief discrepancy between the results for the larger ($N = 149$) and the smaller ($N = 50$) series of varieties is to be seen in the interrelationship for 1906 and 1907 where the two correlations are $.042 \pm .055$ and $.226 \pm .091$. Here the disagreement is apparent rather than real. The difference is $.184 \pm .106$, which can not be considered statistically trustworthy.

factor, (c), it is obvious that if there be only a very slight average percentage infection the test of disease resistance will not be a very critical one, whereas the average percentage can not be very high indeed unless conditions are so unfavorable that all varieties are affected. The percentage infection of early blight varies enormously from year to year. Thus:

	Percentage Infection	
Year	$N = 149$	$N = 50$
1905	3.3	2.4
1906	85.0	83.2
1907	41.7	38.9
1908	—	10.8

With an incidence of 3 per cent. one year and of 85 per cent. the following season one can, in view of the considerations mentioned above, hardly expect to obtain smooth values of the correlation coefficient.

It is interesting to compare these results with those for other maladies of the potato. In the same publication Stuart gives the results of trials for resistance of tubers to scab. Unfortunately the experiments of the second year, 1907, included only 20 of the 65 varieties from the first year. Calculations may be based on the percentage of tubers which are free or nearly free from scab. This is much lower the second year.

	1906	1907
Mean	64.21	28.20
S. D.	11.59	16.33
Correlation	$r = .591 \pm .098$	

The probable error is high because of the fewness of the varieties retained in the second year's test, but the correlation is of more than medium value and is relatively about 6 times as large as its probable error. Thus susceptibility to scab is probably to a very considerable extent a varietal character.

The results for tuber rot tests are not available for successive years, but Stuart has given[4] the percentage of tuber rot in 89 varieties grown on sandy loam and on clay loam soil in 1905. For these I find

	Sandy Loam	Clay Loam
Mean	8.68	39.76
S. D.	11.28	26.85
Correlation	$r = .653 \pm .041$[5]	

[4] Stuart, W., "Disease Resistance in Potatoes," *Bull. Vt. Agr. Exp. Sta.*, 122, Tables VI–VII, 1906.

[5] The value of r given as $.707 \pm .045$ in *Science*, N. S., 38: 402–403, 1913, is deduced from the 62 varieties for which laboratory cultures were available, and from modified percentages. The values agree within the limits of their probable errors.

The correlation of these data, somewhat smoothed by Jones,[6] with a series of determinations of the percentage growth of the fungus on tubers *in the laboratory* has already been determined.[7] For laboratory growth and loss on clay loam, $r = .584 \pm .059$. For laboratory growth and loss on sandy loam, $r = .594 \pm .055$.

Taken as a whole these correlations indicate (*a*) that susceptibility to both early and late blight and to scab differs greatly from variety to variety, and (*b*) that, so far as the evidence goes, the varieties differ more in resistance to tuber injury than to foliage infection by early blight.

It is not at all necessary that the correlations be drawn between the amount of injury to the same organs of the plant or by the same disease. In many instances the so-called cross correlations yield valuable results.

For example Stuart[8] discusses the question of the relationship between vine infection and tuber rot. The point may be subjected to a statistical test by correlating between the maximum percentage of foliage affected by late blight as given in his Table V for potatoes grown on sandy loam soil in 1905 and percentage of rot as recorded in his Tables VI and VII. Unfortunately, the percentages are available for the vines for sandy loam soil only (Table V) while the figures for tuber rot are given for both sandy loam and clay loam soil. Both correlations may be worked out. I find:

For percentage foliage infection on sandy loam soil and per cent. tuber rot on sandy loam soil

$$N = 131, \quad r = .316 \pm .053.$$

For percentage foliage infection on sandy loam soil and per cent. tuber rot on clay loam soil

$$N = 80, \quad r = .102 \pm .075.$$

In both cases the correlations are positive, and hence such evidence as they furnish indicates that the varieties which show the greatest infection of the leaves actually are the worst to rot. That the correlation between injury to the tops and tuber rot is higher on the sandy loam soil is not at all surprising, since the same individual plants—not merely the same varieties—are in-

[6] Jones, L. R., N. J. Giddings and B. F. Lutman, "Investigations of the Potato Fungus *Phytophthora infestans*," *Bull. Vt. Agr. Exp. Sta.*, 168: 74–81, 1912.

[7] Jones and collaborators, *loc. cit.*, and the Reviewer, *Science*, N. S., 38: 402–413, 1913.

[8] *Bull. Vermont Agr. Exp. Sta.*, 122, p. 116.

volved in the correlation. The problem is, however, a complicated one and much more extensive data are needed for a complete analysis.

A problem of very great biological interest as well as of practical importance is that of the specificity of disease resistance. Concretely: Do varieties differ in their susceptibility to a specific disease only, or do they differ merely in susceptibility to disease in general?

A comprehensive and final answer will require far more data than are available and more stringent statistical analysis than can be illustrated here. Some progress can be made by the method of correlation as follows.

If susceptibility be purely *specific* there should be no correlation between the incidence of disease x in year (or culture) p and disease y in year (or culture) q, although there should be a correlation between the incidence of disease x or disease y in different years or cultures. If, on the other hand, differences in disease resistances from variety to variety are determined solely by general weakness or vigor of the stocks, one should expect the correlations between the incidence of different diseases in different years or cultures to be (within the limits fixed by the errors of measurement and the probable errors of random sampling) as high as those between two series of determinations of incidence of one and the same parasite.

Consider first the relationship between the percentage of foliage injury by early blight in 1905, 1906, and 1907 and the percentage of tuber rot in 1905. The correlations are:

Foliage Injury	Per Cent. Tuber Rot on Sandy Loam, 1905 $N = 131$	Per Cent. Tuber Rot on Clay Loam, 1905 $N = 89$
Early Blight, 1905....	$.167 \pm .057$	$.256 \pm .057$
Early Blight, 1906....	$.211 \pm .056$	$.249 \pm .067$
Early Blight, 1907....	$.291 \pm .054$	$.440 \pm .058$

Without exception the correlations are positive in sign. While numerically low, the most of them taken individually may be considered statistically significant in comparison with their probable errors.

Thus it seems clear that the varieties with foliage most injured by early blight are also most subject to tuber rot, just as has been shown to be the case in foliage and tuber infection by late blight.

For foliage infection by early blight in 1905, 1906, and 1907 and foliage injury by late blight in 1905 I find:

	Correlation, $N = 131$
Early Blight, 1905, and Late Blight, 1905	$-.066 \pm .059$
Early Blight, 1906, and Late Blight, 1905	$+.190 \pm .057$
Early Blight, 1907, and Late Blight, 1905	$-.040 \pm .059$

The results are not so consistent as those of the preceding table. The two negative coefficients are insignificant in comparison with their probable errors, and the positive one is not large, either absolutely or relatively. Possibly the laxness of the correlation is in part due to the fact that the measurement of both characters is subject to a large possible error.[9]

For freedom of the tubers from scab with the incidence of other diseases every possible correlation has been determined. The coefficients are shown in the accompanying table. Note that in this case the correlation is between *freedom* from one disease and *occurrence* of another disease. Hence a negative coefficient has the same meaning as a positive one in the foregoing discussions.

FOR PERCENTAGE OF TUBERS FREE OR NEARLY FREE FROM SCAB IN 1906 AND 1907 AND INCIDENCE OF OTHER DISEASES

Diseases Compared with Freedom from Scab	Correlation 1906	Correlation 1970
Per Cent. rot on Sandy Loam Soil, 1905	$-.136 \pm .077$ ($N = 74$)	$-.357 \pm .132$ ($N = 20$)
Per Cent. rot on Clay Loam Soil, 1905	$-.280 \pm .096$ ($N = 42$)	$-.030 \pm .187$ ($N = 13$)
Top Injury by Late Blight, 1905	$+.035 \pm .084$ ($N = 65$)	$-.224 \pm .151$ ($N = 18$)
Top Injury by Early Blight, 1905	$-.561 \pm .054$ ($N = 74$)	$+.240 \pm .142$ ($N = 20$)
Top Injury by Early Blight, 1906	$-.119 \pm .077$ ($N = 74$)	$-.352 \pm .132$ ($N = 20$)
Top Injury by Early Blight, 1907	$-.118 \pm .077$ ($N = 74$)	$-.040 \pm .151$ ($N = 20$)

Because of the small number of varieties involved and the roughness of the measurements the correlations are low and irregular. In ten cases the negative sign indicates that the varieties which are most free from scab are also least susceptible to attacks by other diseases. In neither of the two cases of positive correlation is the constant statistically significant in comparison with its probable error.

Thus altogether 23 of these cross correlations—that is correlations between injury to different organs by the same disease, or

[9] In one case, 1905, the correlation is between foliage injury by two different organisms *in the same year*. What interrelationship is to be expected in such case requires further consideration.

to the same organ by different diseases, or to different organs by different diseases—have been worked out. Only 4 of these—that is only about one case out of six—are exceptions to the rule that varieties which show more than the average amount of injury by one disease will, on the whole, show more than the average injury by another disease. No one of these exceptional constants can be considered significant with regard to its probable error. Several of the 19 which indicate the rule may be looked upon as individually trustworthy. Thus notwithstanding the large variations in numerical magnitude incident to small series of data and rough measurement, the determinations taken collectively certainly furnish highly convincing evidence that to a considerable extent susceptibility to disease is general rather than specific.

The fact that the series of correlation coefficients here presented justify much more definite conclusions than those who have considered the data without statistical analysis have drawn, is sufficient indication of the usefulness of the biometric method in the preliminary stage of disease-resistance experiments in which large numbers of strains are being tested, and in which the mass of data is highly confusing. The special cases illustrated by no means exhaust the possibilities of the biometric formulæ now available. Had the data been more extensive, the analysis might have been carried much farther.

Nothing that has been said in this paper in emphasis of the statistical method must be taken to imply that the most careful individual analysis is not desirable and essential. The two methods are not mutually exclusive, but supplemental.

J. ARTHUR HARRIS

THE DIFFERENT MEANINGS OF THE TERM "FACTOR" AS AFFECTING CLEARNESS IN GENETIC DISCUSSION[1]

IN the analysis of alternative (or segregating) heredity, we find that certain potentialities, such as that of producing a certain color in some part of the soma, appear to be inherited independently of certain other potentialities. We assume that the germ-plasm carries various corresponding genes, factors, or determiners, whose independence in gametogenesis determines the independence of the somatic characters. Cytological study leads

[1] Paper No. 39, University of California, Citrus Experiment Station, Riverside, California.

to some very probable conclusions as to the time and method of segregation of these determiners; though it does not yet enable us to identify with absolute certainty the actual physical unit of segregation, the cytological and genetic evidence indicates strongly that such a unit exists.

In the current chromosome hypothesis, as developed especially by Morgan and his collaborators (Morgan, Sturtevant, Muller, and Bridges, 1915), the material unit of segregation is assumed to be a part of a chromosome. Breaks in two homologous chromosomes at meiosis, with consequent exchange of parts by the pair, presumably occur at certain definite points only. How close these points may be we can not say, but the general stability of Mendelian characters indicates that the number of points is limited. On these assumptions, the portion of a chromosome between two adjacent points of possible breaking is the ultimate physical unit of genetic segregation—essentially a *locus* as defined by Morgan (1915, p. 419).[2]

It is now widely recognized that, in effect, a single real unit of segregation may influence very diverse characters of the soma, often in ways which can not be at all inferred one from another. Very possibly one physical unit of segregation may affect, say, flower color and height in ways just as distinct physiologically as may two distinct units of segregation, although transmission is different in the two cases. In the latter case we say that two genetic factors are concerned; are we compelled in the former case to admit only one?

As a matter of fact, as will be evident on further consideration, either course is possible, according to the definition of *factor* accepted.

If by "factor" we mean a *developmental potentiality,* the delimitation of a particular factor is largely a matter of convenience in analysis. On the other hand, if the term is used to designate a supposed actual physical unit of segregation, a factor has a definite objective extent.

The former view is that of the presence-and-absence terminology, as it is generally understood at present. In this sense, a factor is not an element of the germ-plasm; it is rather a property or characteristic of the germ-plasm or of some element of the germ-plasm. The characters of an organism, as Gates (1914,

[2] Though Morgan, Sturtevant, Muller and Bridges (1915, p. 155) suggest the possibility that the loci of linked factors may be so near together in a chromosome "that they never (or very rarely) cross over." The definition of *locus* is discussed below.

p. 269) has remarked, are "attributes," no more to be separated from the organism than are the properties of a chemical compound from that compound. The factors of the presence-and-absence scheme, similarly, are inferred *properties or attributes of the germ-plasm*, by whose behavior we explain the alternative transmission of certain properties or attributes of the soma.

Obviously an organism is not composed of "characters"—and neither is its germ-plasm composed of "factors," so long as the factors are those of the presence-and-absence scheme. Such factors are nothing but *characters of the germ-plasm*, and, like the characters of the soma, they are more or less conventionalized in description. We have no warrant for projecting these conventionalized descriptions back into the actual germ-plasm, and assuming the presence and absence there of strictly corresponding material units of segregation.

The presence-and-absence scheme, when not encumbered with non-essential hypotheses, is a strictly neutral instrument of genetic analysis. If there is segregation in the formation of the germ-cells, it is merely a matter of definition to state that a factor is allelomorphic to its absence. That is, the assumption of segregation is the only assumption required by this scheme, which is the logically simplest form of the "conceptual notation" (East, 1912) of genetics.

Very special emphasis must be placed on the fact that the "absence" is absence of a *potentiality*, without reference to the presence or localization in the germ-plasm of any other potentiality that may actually take its place. The allelomorphism of the presence-and-absence notation is a *logical* opposition; when it makes "A" and "no-A" allelomorphs, this involves no assumption as to what may be physically opposed, in the chromosomes, to the physical basis of "A."

If an "absence" a of a given factor A is always or commonly accompanied by an actual presence of a corresponding factor A', and we wish to represent this fact, it is provided for by the terminology of linkage; we may use $\widehat{Aa'}$ and $\widehat{aA'}$, since the presence-and-absence scheme makes no assumptions as to the structure of the germ-plasm. It is obviously simpler to write simply A and A', or A and a, for the two factors, and convenience may justify this practise; we should note, however, that in thus abandoning the presence-and-absence terminology we introduce a second assumption, that of actual factor-to-factor opposition or allelomorphism. This assumption is, of course, in view of all the evidence, a highly probable one, and especially convenient

in cases of apparent multiple allelomorphism (Morgan, Muller, Sturtevant, and Bridges, 1915, chap. 7). In fact, this added assumption probably permits a more direct and therefore practically simpler representation of the actual course of segregation. Whether the corresponding factor-to-factor notations now used for Drosophila (Castle, 1913; Morgan, Sturtevant, Muller, and Bridges, 1915, p. 233) are everywhere adequate and convenient is another question, as Emerson (1913) has shown.

Cases of multiple allelomorphism involve no special difficulty *in principle* for the presence-and-absence scheme. They can of course be represented only by linkage formulæ, in which the "presence" of one factor of the set is linked with the absence of the rest—but their very occurrence suggests that we might, as is suggested above, correctly enough represent single factorial differences in the same way. All this can affect only the convenience of the notation, and not at all its logical applicability.

If we adopt a factor-to-factor system of notation, it is natural to conceive of the opposed "factors" not as mere potentialities, but as physical units responsible for genetic potentialities. When we have taken this viewpoint, we have begun to use the word *factor* in the second sense mentioned above; we are thinking of assumed physical units of segregation, not merely of observed potentialities of development. Morgan (1915, p. 419), in discussing "presence and absence," uses *factor* in this sense, as do Morgan, Sturtevant, Muller, and Bridges (1915, pp. 220–222).

No doubt what has been said above is an old story to experienced geneticists in general, in view of such discussions as those of East (1912) and Morgan (1915). The distinction is so fundamental, however, and the double use of the term *factor* so increases the difficulties of the case, that consideration of the general problem from the present terminological viewpoint seems highly desirable. Perhaps greater precision in the use of several terms could be attained.

Johannsen (1909, pp. 124–125), in defining the term *Gen* (gene), makes it perfectly plain that he means the material basis[3] or cause (the *Anlage*), of whatever sort, of a "unit character," defining a *unit character* as one dependent on a special kind of gene. He is evidently inclined to consider the gene as the material unit of segregation, holding that the sum of the genes constitutes the germ-plasm, and there is a widely prevalent

[3] Or *immaterial* basis, if we must admit the theoretical possibility of the existence of immaterial "entelechies" associated in some more or less mystical way with the germ-plasm.

tendency among geneticists to use *gene*, and its synonyms *factor* and *determiner*, in this sense, which is the second of the two discussed above. Evidently *gene* is not properly used in the first sense. A moment's thought, however, will show the impracticability of confining *factor* to that sense; the meaning of this term shifts back and forth continually in common usage, and often remains indefinite.

Further, when *factor* (or *gene*) is used in the second sense, we consider it coextensive with *locus* (Morgan, 1915, pp. 419–20; Goodspeed and Clausen, 1917, p. 32). A factor is a particular state or condition of a locus. Let us, then, define *locus* as the physical unit of segregation, almost certainly identified as a genetically indivisible portion of a chromosome. Genetically inseparable ("completely linked") potentialities, then, belong to the same locus, and hence to the same factor or gene; "completely linked factors" are mainly[4] relegated to non-cytological discussion, and especially to use with the presence-and-absence terminology.

No doubt Mendelian analysis considers, in any case, only some of the most readily identifiable properties of the real units of segregation concerned, and this fact seems to deserve a large place in our genetic thinking. Especially is it important that the two meanings of *factor* and its synonyms should be clearly distinguished; when these meanings are unconsciously interchanged and confused, vagueness and misunderstanding are sure to result.

The student of genetics may read, for example (East, 1912),

> If we forget ourselves and begin to speak of unit factors as particles, only a confusion follows similar to that caused by Nägeli, Spencer, and Weismann. Nothing is gained and even facts are obscured.

On the other hand, he will find the factors of *Drosophila* located with mathematical exactness in diagrams of the chromosomes, and often apparently or explicitly considered as material components of the chromosomes. In the interest of clear thinking, especially in the case of beginners and casual students in the field of genetics, the explanation of this apparent contradiction deserves very special emphasis.

As an example of the way in which this terminological conflict may cloud an argument when the essential facts are clear to the writer, we may take the following case. Morgan (1913,

[4] Obviously, temporarily complete linkage of genes must be excepted, as in the usual case with the male of *Drosophila*. The point is that the chromosome theory may well consider, in any case, that potentialities *always* associated are manifestations of the same factor or gene.

p. 10), in urging his substitute for the presence-and-absence scheme, largely on grounds of convenience, agrees with East (1912) as to the general usability of the latter scheme as a "system of nomenclature" without cytological implications. He makes especially plain the undesirability of interpreting "absence" as a physical absence in the germ-cell, or (1915, p. 419) as "a hole in a chromosome." In one respect, however, Morgan's discussion seems less clear than it might be, and this is in the use of *factor*, in these articles, in a sense (the second here) which is not that of the presence-and-absence scheme, with only vaguely implied explanation of the distinction. It certainly *is* permissible to speak of "the absence of a factor from the germ-plasm," *if we mean the kind of "factor" implied by the presence-and-absence terminology.*

We must make it as clear as possible that *factor* (1) sometimes means a potentiality and (2) sometimes means a body, and that a factor is assumed to be paired either (3) with its absence or (4) with another (identical or different) factor. The combination of (1) and (3), then, gives the presence-and-absence scheme, while the combination of either (1) or (2) with (4) gives the scheme used by Morgan (1913; see also Castle, 1913) and other students of *Drosophila*. General objections to Mendelian analysis have been based largely on confusion of (1) and (2), which often leads to erroneous suppositions—for instance, that Mendelian analysis in general, or the presence-and-absence method of Mendelian analysis, requires the unnecessary and unwarranted assumption involved in the combination of (2) and (3). Probably this confusion is also largely responsible for the persistence of another often discredited notion, the idea that "Mendelians" suppose their factors to be individually the basis of somatic characters, rather than simply necessary elements in an interacting complex which produces the characters.

Summary

The term *factor* has, in genetic use, two distinct meanings, which are continually interchanged or combined and often confused. It is essential to clearness in genetic discussion that these two meanings should be carefully distinguished. These meanings may be indicated by the following formal definitions:

1. A genetic (Mendelian) factor is a property or characteristic of the germ-plasm, more or less conveniently delimited for the purpose of analysis of segregating heredity.

2. A genetic (Mendelian) factor, or gene, is an actual material

unit of genetic segregation; it is of unknown nature, but probably consists of a genetically indivisible portion of a chromosome (a *locus*) in a particular state.

The presence-and-absence scheme of factor notation properly employs only the first of these meanings; the Morgan-Castle scheme, on the other hand, may use either.

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THE SELECTION PROBLEM

UNLESS history fails to repeat itself, geneticists, whose attention is focused upon variation, should sooner or later overemphasize its importance as a factor in shaping the organic complex. There is, indeed, reason to believe that already a tendency for some among them to do so is becoming apparent. Dr. Pearl's[1] recent paper under the title above affords an example in point.

In that communication its author fails to discriminate sharply between two distinct phases of his subject. Whether selection may affect the course of evolution is a matter entirely apart from the possibility that it alters the germ plasm. Racial history may possibly be modified, if the genetic composition of a mixed population may be affected by selection based upon somatic dif-

[1] Pearl, Raymond, 1917, "The Selection Problem," THE AMERICAN NATURALIST, Vol 51, pp. 65–91.

ferences. But modification of the germ plasm by selection is impossible, if that agency acts only as "a mechanical sorter of existing diversities."

The present note is not concerned with the causes of variation. It refers only to the first-mentioned phase of the selection problem. It accepts the statements in the quotation below as substantially correct, and attempts in brief compass to evaluate the arguments by which Dr. Pearl supports his position regarding them.

By transposition of a few phrases his ideas may be expressed in his own words as follows:

> The mere fact of elimination and survival . . . is capable, in theory at least, of bringing about evolutionary changes of a progressive sort, . . . if the elimination be selective, and the survivors transmit to their progeny those differences that mark them off from the eliminated. The theory that these two rules are always and everywhere in operation, taken together with the observed fact that living creatures do die, *is* the Darwinian theory of Natural Selection as a factor in organic evolution.

If, as is implied, Darwin gratuitously assumed the intolerable burden involved in the use of the words, *always* and *everywhere*, it is immaterial. It is not a vital issue whether the form in which he expressed himself will bear literal interpretation, but merely whether his idea is correct that natural selection effects notable changes in the course of evolution. Hence it seems sufficient to say, that if the "Dance of Death" is governed in general, or even in part, by the joint action of the two principles enunciated in the preceding paragraph, the changes described there should follow as surely, although more slowly, than if the conformity were complete.

It is stated by Dr. Pearl, as one of three broad facts on account of which natural selection is no longer regarded as a primary, or perhaps even a major factor in evolution, that even when selective elimination on the basis of somatic characters does occur, it does not follow generally and regularly that the somatic differences on which the selection acted will reappear in the progeny, . . . actual experience having abundantly demonstrated that a very great many of such somatic differences are not inherited.

This may refer, first, to the fact that a single phenotype may include members of different genotypes. Yet, even so, no strict limitation is placed upon the possibility of changing the character of a mixed population by selection based upon somatic qualities. If plus variants of an inferior strain seem superior to

individuals of a higher order of genetic worth, this will have its due effect in impeding progress; but since the essential point is, that, collectively considered, members of different pure lines do, in general, rather definitely reflect their different germinal constitution, advance may undoubtedly be made. However, it is unnecessary to labor the point, as it is not only admitted, but is urged by geneticists themselves in explanation of such results, for example, as those Castle and Phillips[2] obtained in selection experiments with hooded rats.

Reference to the genetic behavior of such characters as side-sprig of the comb in poultry may also be included in the quotation above. If, in this case, emphasis is laid upon the fact that selection might apparently be exercised indefinitely without the least tendency toward evolution of a side-sprigged race, it needs only be pointed out that the argument is directed against the contention that selection is capable of modifying the germ plasm. It has no bearing whatever upon the possibility of its occurrence in nature, nor, aside from the point indicated, upon its influence in evolution.

Upon the other hand it seems quite impossible that even if characters of which side-sprig is representative were highly useful, they should fundamentally modify the course of evolution under selection, or place any notable obstacle in its way, for they are not germinal variations and are as likely to occur in one line of descent as another. Hence, other things being equal, it is quite as probable that they should assure the survival of a subnormal representative of a superior genotype, as that they should tide over a superior representative of an inferior one; and the chance would be no greater that the attribute in question should appear in the offspring of one rather than in that of the other.

A second of the three general groups of facts to which reference has been made is summarized in the statement:

> Observation indicates that in many cases evolutionary changes have come about by relatively large, discontinuous steps, the new form being not merely fully differentiated at its first appearance, but also fully able to survive.

In another connection it is stated forcefully by Dr. Pearl that if the game of survival is actually played by the quoted rules he formulates (and no others are necessary) the conclusion is logically irresistible that progress is bound to occur in the direction of those differences which distinguish the survivors. But

[2] Castle, W. E., and Phillips, John C., 1914, Carnegie Institution of Washington, Publication 49. Castle, W. E., 1916, "Genetics and Eugenics," Harvard University Press, Cambridge, 8vo, pp. vi + 353.

since obedience to his two rules is not in the least contingent upon the magnitude of the variations upon which selection is based, it must be admitted that the facts summarized above are entirely irrelevant to the present discussion. They neither bear upon the phenomena of inheritance, nor add anything to our knowledge of selective elimination.

The third general fact cited in support of Dr. Pearl's contention regarding the diminished esteem in which natural selection is held as a factor in evolution is:

> All organisms possess in varying, but usually in very large, degree the power of personal, immediate, individual, somatic adaptation to the environment.

It is affirmed in addition, that in consequence of this power of personal adaptation the survival expectation of an individual is not generally and regularly a function of any static, single-valued relation between its somatic structure, habits or physiology, on the one hand, and the impinging environmental stresses on the other. Yet, it is asserted, such a relation is implicitly assumed in that part of the theory of natural selection which affirms a selective elimination on the basis of somatic characteristics.

The reply to these various statements is, that their substantial truth may be admitted without the possibility that evolution is affected by selective elimination being thereby in the least diminished. The adaptive capability of any individual either rests upon or lacks a germinal basis. In the former case there is no obvious reason why it should not itself provide material upon which selective elimination might be based, with consequent change in the composition of the population. In the latter, individual adaptability is as incapable of exercising influence upon the course of evolution, as side-sprig should be, if it were a useful character of the same order of importance.

If the accumulated results of genetic research provide no more effective arguments than these, it must remain an open question whether natural selection is not a primary factor, not in the *origin* of species, but in the determination of the elements composing the flora and fauna of the world at any period in its history. In other words, one who wishes to force an abandonment of that position must demonstrate that selective elimination does not occur upon such a scale that it may account for the results ascribed to it.

For many legitimate reasons Dr. Pearl has not treated this point at length in his article under discussion. But from the scanty evidence available he derives the following conclusion:

In some cases natural elimination is certainly in some degree selective, while in other cases it certainly is not, and in the most favorable cases of all the selection is apparently not very rigorous. Gross teratological abnormalities are eliminated. But the smaller deviations from type, which in theory ought to furnish the basis of selection, appear upon quantitative study less generally and sharply determinative of survival than might have reasonably been expected theoretically.

It is perhaps worthy of note that ever since it appeared that the larger, and rarer, discontinuous variations are in no danger of being lost through swamping, it has been beside the mark to ascribe especial theoretical significance to smaller deviations from type. Attention may also be directed to the important admission that in some instances elimination is known to be selective. It will then be in order to examine reports of researches upon the basis of which it is confidently asserted that in other cases the same is not true.

It happens that with five others one is cited which falls squarely within my own field of investigation. This is Professor Reighard's[3] "Experimental Field Study of Warning Coloration in Coral-reef Fishes," which Dr. Pearl seems to consider of particular significance for his own argument. But since it has been my good fortune to study the same material in the same place, more extensively and with better facilities, I feel justified in saying that Reighard's results will bear no such interpretation as is here placed upon them. He proved that gray snappers possess powers of discrimination and of memory which would lead one to suppose that, if the bright colors of the smaller reef fishes possess a warning significance, the snappers should be aware of it and avoid them. He did not prove beyond possibility of doubt that they do attack such fishes freely, but that is of no importance in the present connection; his conclusion that tropical fishes are not warningly colored with reference to their commonest enemies is perfectly sound. His ideas regarding immunity coloration are, however, the only ones in his paper which have the remotest bearing upon the matter of selective elimination, and there is no reason to suppose that these are more than logical deductions from incorrect premises.

Dr. Pearl's[4] own report upon the natural elimination suffered

[3] Reighard, Jacob, 1908, "An Experimental Field-study of Warning Coloration in Coral-reef Fishes," Carnegie Institution of Washington, Papers from the Tortugas Laboratory, Vol. 2, pp. 257–325.

[4] Pearl, Raymond, 1911, "Data on the Relative Conspicuousness of Barred and Self-colored Fowls," THE AMERICAN NATURALIST, Vol. 45, pp. 107–117.

by barred (Plymouth Rock) and black, or near-black, chickens living under the same conditions is also mentioned. In this paper photographs show the black fowls looming up against natural backgrounds much more distinctly than do the barred. This is accepted as "objective and unbiased evidence regarding the relative conspicuousness of the two types of plumage pattern." It follows naturally, since the extensive record shows little difference in the rate of elimination of the two sorts of birds, that "the relative inconspicuousness of the barred color-pattern afforded its possessors no great or striking protection against elimination by natural enemies." But photographs serve as *accurate* measures of the conspicuousness of the fowls in the eyes of color-blind enemies only. Therefore, if rats and predaceous birds are not color-blind, and there is perfectly good reason for supposing that creatures lower in organization than either have color vision, it is not at all certain that the elimination in the two cases does not correspond fairly well with the actual difference in conspicuousness of the two types.

Kellogg and Bell's[5] interesting "Studies of Variation in Insects" deals with 24 species. In 23 of them, including the lady-beetle, *Hippodamia*, to which Dr. Pearl refers, the authors show that there is much variation in individuals which have successfully run the gauntlet of natural selection. But since they have no knowledge whatever of the variation in the original populations, of which they have studied survivors only, these their results show nothing regarding the extent, or even the occurrence, of selective elimination.

In the honey bee alone duplicate studies were made of the variation of certain structures in individuals which were about to hatch, and in others, apparently from the same hive, after exposure to the vicissitudes of an active life. Among 200 drones in the first group the veins of the fore wings in 11 were imperfectly developed, and as a result normal flight became difficult or impossible. The variation in others of the first series seems essentially the same as that observed in the 300 members of the second, among which none of the defective individuals were found. But if these facts prove anything, it seems to be that selective elimination does occur when unfavorable variations affect the normal functioning of an organ. Suggestion is entirely lacking that the mechanical efficiency of the wing is impaired by the other variations noted, and it can scarcely be con-

[5] Kellogg, V. L., and Bell, R. G., 1904, "Studies of Variation in Insects," Proc. Washington Acad. of Sciences, Vol. 6, pp. 203–332.

sidered a pregnant fact, in the present connection at least, that indifferent variations provide no basis for selective elimination.

In the same two series of bees the variation in the number of hooks upon the costal margin of the hind wings was determined without significant difference appearing in the two cases. In this instance, since the hooks appear to operate to the insects' advantage in binding the fore and hind wings of each side together, it seems plausible enough at first glance, that the more hooks there are the more efficiently their function will be discharged. Upon second thought, however, a difficulty suggests itself. The number of hooks varies from 19 to 29 in different individuals, but even the smaller number may, for all that is known to the contrary, perform perfectly the function ascribed to them. In that event the others are superfluous and the advantage they confer entirely fictitious. But waive the objection, and what follows? Simply a conclusion which in its relation to the present argument is already invalidated: Variations of the *magnitude* indicated provide no "handle" for natural selection.

It is unnecessary to carry the examination of the evidence farther. The three papers which have been reviewed are not carefully selected for criticism, but are the last, and apparently the most important, of six certified to be "fairly representative." If, however, this characterization is correct, it is apparent that the case against selective elimination is greatly exaggerated.

In conclusion, it appears that neither genetic research nor studies upon elimination closely limit the possibility that selection has played a very important part in evolution. In addition, recent field-studies[6] demonstrate novel facts of common occurrence which must apparently be ascribed to the action of this factor. Hence as was suggested in the beginning, Dr. Pearl would seem to over emphasize the importance of variation, and to attach too little significance to selective agencies in determining the course of racial history.

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[6] Longley, W. H., 1914, Report upon color of fishes of the Tortugas Reefs, Carnegie Inst. of Washington, Year Book No. 13, pp. 207–208; 1915, "Coloration of Tropical Reef Fishes," Carnegie Inst. of Washington, Year Book No. 14, pp. 208–209; 1916; "The Significance of the Colors of Tropical Reef Fishes," Carnegie Inst. of Washington, Year Book No. 15, pp. 209–212; 1916, "Observations upon Tropical Fishes and Inferences from their Adaptive Coloration," *Proc. Nat. Acad. Sciences*, Vol. 2, pp. 733–737.